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DOES THE INTERVAL OF DELAY OF CONDITIONED RESPONSES POSSESS INHIBITORY PROPERTIES?[1]

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PROBLEM

The recent publication of Lepley's hypothesis (**15, 16**) concerning the relationship between the remote excitatory tendencies in rote learning (**3, 4, 23**) and trace conditioned responses, and the incorporation of this hypothesis by Hull (**11, 13**) as a postulate in a deductive system accounting for many of the phenomena of rote series learning, have called attention to the probable importance of trace conditioned responses in human learning.[2] The extensive use of this postulate makes particularly urgent the confirmation on human subjects and under controlled conditions of Pavlov's (**17**) contention that the interval of delay of delayed and trace conditioned responses possesses inhibitory[3] properties capable of being generalized to other reactions which might occur during this interval.

[1] This investigation was carried out in the laboratory of Professor Clark L. Hull. The writer wishes to express his indebtedness to Professor Hull for assistance and criticism. This study is an abridgment of part of an investigation presented to the Faculty of the Graduate School of Yale University in partial fulfillment of the requirements for the degree of Doctor of Philosophy in psychology.

[2] Certain of these deductions based on the 'inhibition of delay' have recently been put to experimental test (**9, 10, 12, 13, 20**).

[3] Throughout this paper the term 'inhibition' is used in a strict behavioral sense as a decrement in amplitude or an increment in latency of a response, with no implications of the neuro-physiological nature of the underlying phenomena.

The present investigation was specifically designed to determine whether the 'inhibition of delay' is actually a genuine phenomenon with human beings. This inhibition should be observable as a decrement in the amplitude of a reaction inserted in the interval of delay.

The general procedure for the investigation of this problem requires first the establishment of a delayed conditioned response,[4] which under the conditions of the present experiment occurs about 8 to 10 seconds after the onset of the conditioned stimulus. This is shown diagrammatically in Fig. 1, where the two lines immediately below the time line

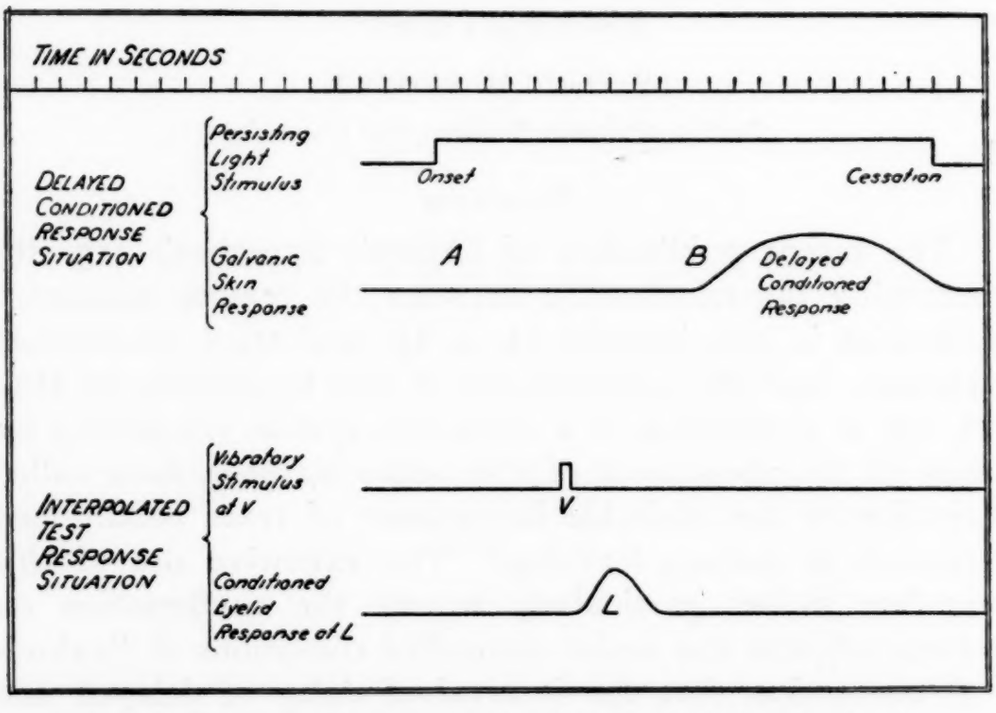

FIG. 1. Diagrammatic representation of the method of testing for the 'inhibition of delay.'

represent an established delayed conditioned response (*e.g.*, galvanic skin response) to a persisting stimulus (light). The interval *AB* is the 'interval of delay.'

Now, if the interval of delay actually possesses inhibitory properties which are generalized to other reactions, another conditioned response such as an eyelid reaction occurring in

[4] A trace conditioned response was not used in this experiment since the writer has found that delayed conditioned responses are established at a considerably faster rate (18).

the interval of delay (*AB*) should be weakened below its normal magnitude. In the two lower lines of Fig. 1 are represented the stimulus for the lid reaction (*V*, a vibratory stimulation of the skin) and its conditioned response (*L*), both occurring midway in the interval of delay, *AB*.

Since the lid response in this situation is designed to serve as an indicator of the presence of a generalized inhibition, it may be termed the *test* conditioned response. In order to determine whether the interval of delay really depresses this test response, the magnitude of the reaction when occurring during the period of delay (*AB*) of a delayed conditioned response must be compared with its magnitude under normal conditions. The normal magnitude of the test response can be found by presenting the stimulus for the lid response a few minutes before or after the interval of delay of the delayed conditioned response. The presentation of the stimulus for this lid response when it precedes the testing (period of delay) situation may be termed the pre-test control; when it follows it may be called the post-test control. A test cycle thus comprises the successive presentations of the pre-test, test, and post-test situations. If there is actually an 'inhibition of delay,' the amplitude of the test conditioned response should be smaller in magnitude than the mean of the pre-test and post-test responses.

Apparatus

Figure 2 is a diagrammatic representation of the entire apparatus. The subject sat in a comfortable Morris chair, completely enclosed in a darkened cabinet. A 60-cycle A.C. current through headphones furnished a hum which effectively masked possible extraneous noises and cues.

Galvanic Skin Response.—The conditioned stimulus for the galvanic skin response (delayed conditioned response) was a patch of faint light (*L*) from a 6-volt flashlight bulb mounted in the front panel of the cabinet. This stimulus, dimmed by a layer of black sateen, had a duration of 21.3 seconds.

A moderate tetanizing shock delivered to the right wrist served as the unconditioned stimulus. The current was derived from a 6-volt lead storage battery and passed through a Cambridge inductorium. The shock had a duration of .625 second and was presented 20.1 seconds after the onset of the light in such a way that the latter continued for about .6 second beyond the termination of the shock. This is illustrated in Fig. 3 (*cf.* light and shock stimuli only), which is a transcript of part of a typical record.

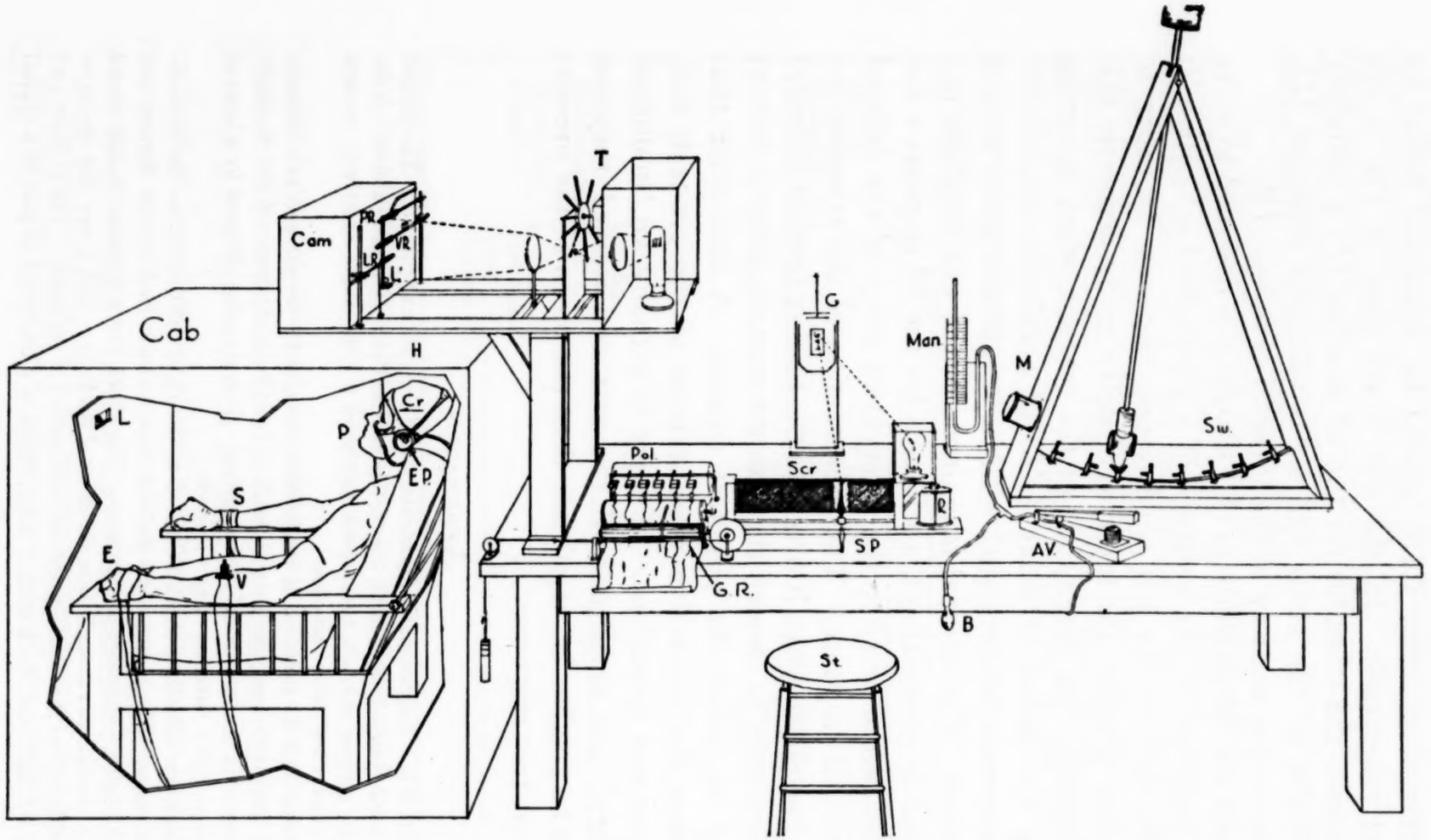

FIG. 2. Diagrammatic representation of the apparatus arrangement. *Cab*, cabinet; *EP*, earphones; *Cr*, crown; *L*, *L'*, light; *E*, galvanometer electrodes; *V*, vibrator; *S*, shock electrodes; *P*, air-puff nozzle; *Cam*, camera; *T*, light source; *PR*, air-puff recorder; *VR*, vibrator recorder; *LR*, eyelid recorder; *H*, hair; *Pol*, polygraph; *G*, galvanometer; *R*, resistance; *Scr*, screen; *SP*, sliding pointer; *GR*, galvanometer recorder; *B*, rubber bulb; *AV*, air-valve; *Man*, manometer, *M*, magnet; *Sw*, pendulum switches; *St*, experimenter's position.

The Tarchanoff phenomenon of the galvanic skin response was recorded. Two silver electrodes (*E*) the size of quarter-dollars were strapped about one fourth inch apart against the palm of the left hand and were connected to the D'Arsonval galvanometer (*G*). The tracing of the galvanometer movement was recorded upon the moving waxed paper of the polygraph (*Pol*) at a reduction of 4 to 1.

Eyelid Response.—Photographic recording of the eyelid response was used. A weak tactual-vibratory sensation having a duration of .24 second served as the conditioned stimulus for the eyelid response (interpolated test response). This was delivered to the right forearm by an electrical stimulator (*V*), described by Bass and Hull (1). The stimulator was actuated by a 4-volt 60-cycle A.C. current and delivered 120 pressures per second.

Reflex eyelid closure was obtained by directing a stream of air against the lateral margin of the left cornea, in a manner similar to that employed by Hilgard and Marquis (7). An air pressure equivalent to 55 mm of mercury in a closed-tube manometer (*Man*) was built up behind an electrically operated valve (*AB*) by means of a rubber

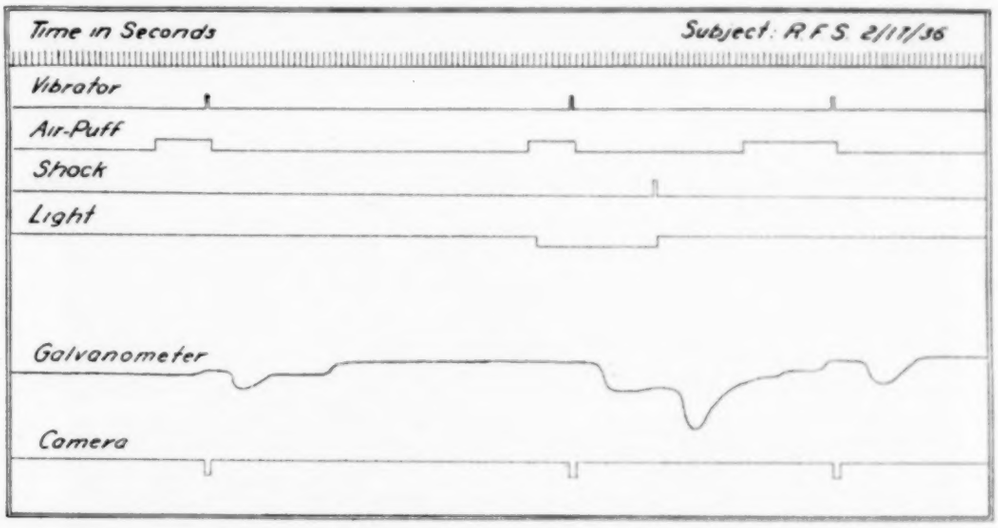

FIG. 3. Tracing of a section of a typical stylographic record showing a complete test cycle: the vibrator-puff combination at the extreme left (pre-test control), the vibrator-puff in the delay period of the delayed conditioned response evoked by the light stimulus (test), and the vibrator-puff at the extreme right (post-test control). The air-puff valve was released at the down stroke of the notch.

bulb (*B*). Upon release of the valve, a stream of air passed through a rubber tube at the end of which was a rubber-tipped glass nozzle (*P*) placed about 1 cm from the cornea. The intensity of the stimulation was sufficient to evoke complete closure of the eyelid. The stimulus was recorded simultaneously with its presentation by diverting one half of the air-stream against a paper marker in front of the camera aperture.

For most subjects, the air-puff was presented 500 milliseconds after the onset of the vibrator. In a few cases, however, when the conditioned response did not reach its maximum amplitude within this period, the interval between the stimuli was lengthened to 640 milliseconds.

Shadow recording of the winks was accomplished by means of a very slender bamboo rod (*LR*) supported at one end by a flat celluloid spring. A human hair (*H*)

corder; *B*, rubber bulb; *AV*, air-valve; *Man*, manometer; *M*, magnet; *Cy*,

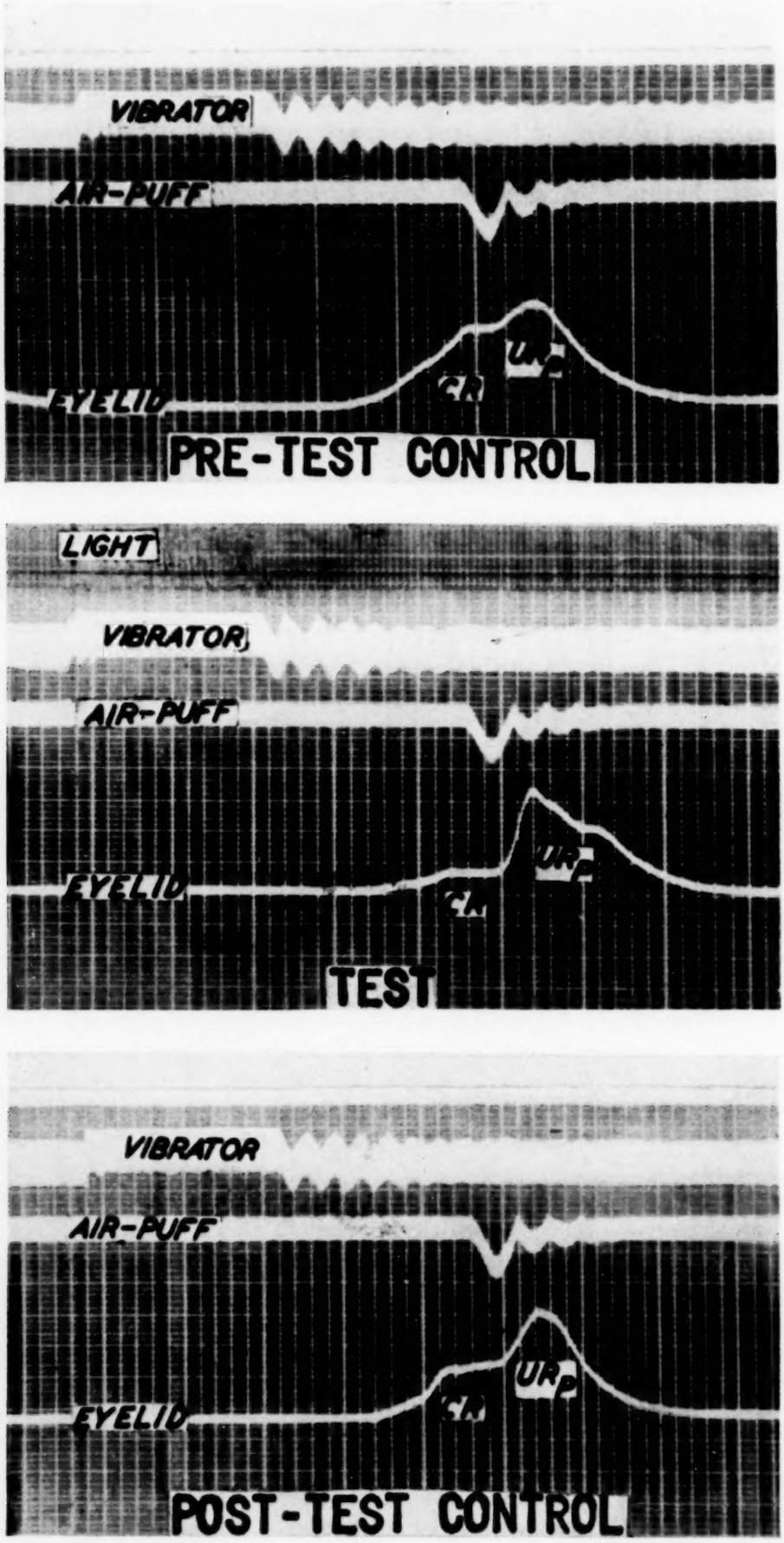
VIBRATOR
AIR-PUFF
EYELID
CR
UR
PRE-TEST CONTROL
LIGHT
VIBRATOR
AIR-PUFF
EYELID
CR
UR
TEST
VIBRATOR
AIR-PUFF
EYELID
CR
UR
POST-TEST CONTROL

FIG. 4.

cemented to the subject's eyelid passed straight upward through an opening in the roof of the cabinet to the bamboo marker. Tension of the hair was regulated by vertical adjustment of the bamboo rod. Photographic calibration of this recording system showed that the bamboo recorder followed the motion of the eyelid with no observable distortion, inertia, or overthrow. The subjects reported that the resistance of the recorder to the lid movement could not be felt. Amplification of the eyelid movement was about two to one.

A camera of special construction was employed to photograph the eyelid movement only during a stimulation. Hilgard (5) and Hilgard and Marquis (7) have shown that the use of an interval between the conditioned and the unconditioned stimuli permitted the conditioned response, due to its tendency to antedate the onset of the reinforcing stimulus, to precede the point of reinforcement. The latency and amplitude of the conditioned response can thus be accurately measured without the omission of the reinforcing stimulus. This anticipation of the eyelid response is shown in Fig. 4.

The timing of the stimuli and operation of the camera were controlled by the combined action of a synchronous motor timer and a heavy pendulum. The stimuli for the eyelid response and the period during which the camera was operating were recorded stylographically, as shown in Fig. 3. The photographic and the stylographic records supplemented each other synchronously in such a way that it was always possible to tell to what point in the continuous polygraph record each individual photographic exposure referred—a matter indispensable for the interpretation of the results. Immediately after their development the photographic records were keyed up with the polygraphic accompaniments by appropriate notations in pencil.

Procedure

The general procedure adopted to investigate the alleged 'inhibition of delay' consisted of the following three steps:

1. The eyelid response was conditioned.
2. A satisfactory delayed conditioned response was established.
3. Tests of the inhibition of delay were made by presenting the stimulus for the conditioned eyelid response in the interval of delay. The magnitude of this lid response was compared with those of the pre-test and post-test control lid responses completing the test cycle. Typical records of such a test cycle are shown in Figs. 3 and 4.

Twenty male university students, paid by the hour, were employed on two successive days in the main experimental group. Two other groups of twenty subjects each served in the control experiments. The subjects were directed to look in the

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Fig. 4. Specimen photographic records of a complete test cycle. The test record shown in the center was taken during the interval of delay of a delayed conditioned response, 5 seconds after the onset of the light stimulus. The pre-test control was taken 85 seconds prior to and the post-test record 45 seconds following this testing situation. It is seen that the conditioned response (*CR*) occurs before the unconditioned response (UR_P) and is considerable smaller in magnitude. Horizontal lines measure amplitude in mm. The fine vertical time lines are 20 ms and the leavy lines 100 ms apart.

By comparing the test conditioned lid response with the mean of the controls the effect of the interval of delay may be determined. In this figure, the persisting light stimulus depresses the amplitude of the test response to an extent of 71 percent. The increment in latency is 9 percent.

general direction of the light throughout the experiment, and to be sure to gaze at the visual stimulus during each presentation.

Session I. One Hour.—On this session, which was the same for both the experimental and the control groups, the conditioned lid response was established. Vibrator-puff combinations were presented until the subject showed a fairly consistent conditioned eyelid response for about 10 successive trials. Thirty-five trials were usually necessary to reach this criterion (range 16 to 50 trials).

Twenty-six subjects were dismissed because the conditioned responses did not meet the consistency criterion within 50 trials. To prevent distortion of the photographic records, eight subjects were rejected for winking faster than a rate of 20 winks per minute. Eight additional subjects were dismissed because of maximal responses, since slight inhibitory or facilitative effects could hardly be apparent if the lid responses were full closure. For this reason, only enough training trials were given to establish a consistent conditioned response without overtraining.

Session II. Two and One Half Hours.—The second session was divided into two parts. The control series was designed to determine to what extent the persisting light stimulus, before any conditioning to the shock had occurred, would influence the conditioned lid reaction. The experimental series, which followed immediately, comprised the training trials for the delayed conditioned response and the tests to determine whether the interval of delay was actually inhibitory. On all test trials the vibrator-puff combination was presented 5 seconds after the onset of the light. Vibrator-puff combinations both preceded and followed each test trial (pre-test and post-test controls), and were scattered among the presentations of the light. In this way the subject was unable to identify the test trials prior to actual stimulation.

A formal description of the procedure is as follows:

A. Control series.

(1) 3 presentations of vibrator-puff combination. (10 presentations if the conditioned eyelid response was quite weak.)

(2) 10 presentations of the unreinforced 21.3-second light. Presentations 1, 5, and 10 were test trials.

B. Experimental series.

(3) 3 presentations of simultaneous light-shock combinations.

(4) 20 presentations of 21-second light-shock combinations. Presentation 16 of the 21-second light-shock combination was a test trial.

(5) 15-minute rest interval (subject allowed to exercise).

(6) 28 presentations of 21-second light-shock combination. Presentations 36, 40, 45, and 48 were test trials.

Development of the Conditioned Eyelid Response

Photographic records of the development of the conditioned eyelid response during Session I were taken on alternate stimulations with 39 of the 60 subjects used in this investigation. The records of the other 21 subjects were not measured, since they were taken at irregular intervals and served merely to inform the experimenter of the general progress of the development of the conditioned eyelid reaction.

Some of the subjects showed a maximal response after the first few reinforcements. If, upon being questioned, these subjects replied that they were deliberately anticipating the air-puff, they were instructed not to do so; if the eyelid response to the conditioned stimulus then reappeared as a less than maximal response the subjects were retained for the main part of the experiment on the second session. As has

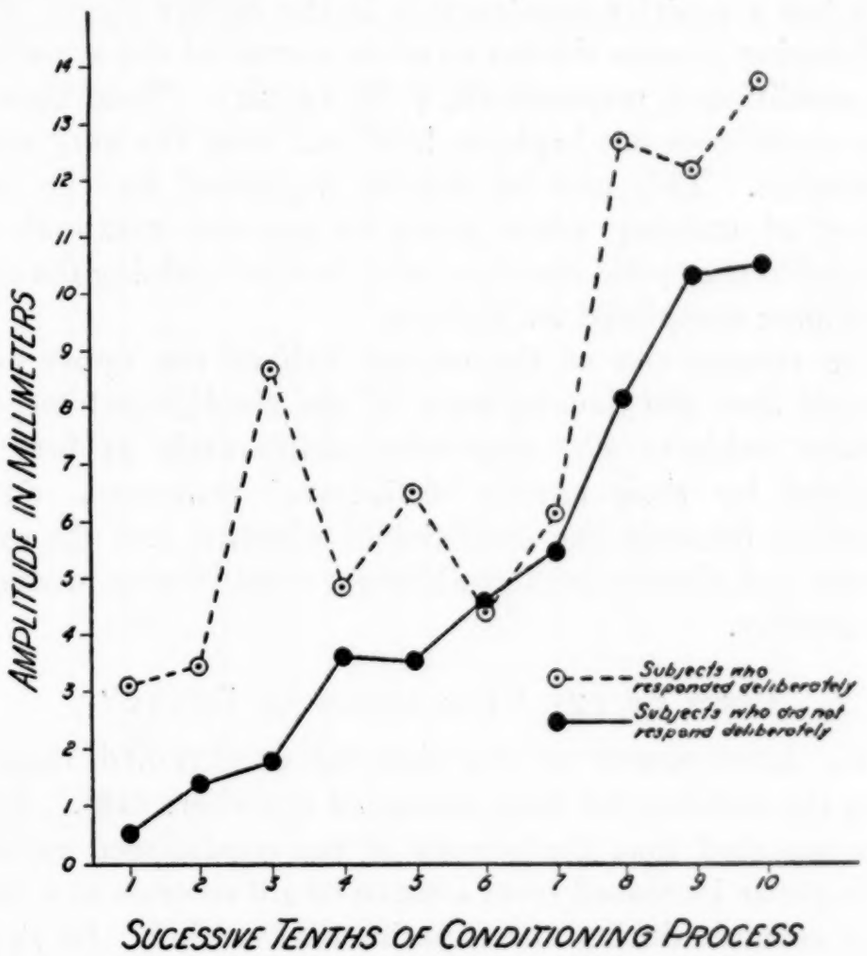

FIG. 5. Composite Vincent curves showing the increase in the amplitude of the eyelid response during conditioning.

already been pointed out, this method of selection was necessary in order to avoid using subjects who were responding maximally. With 13 subjects who admitted closing their eyes deliberately to avoid the air-puff, this wink dropped out and later reappeared as a less than maximal response. The upper curve in Fig. 5, based on the data from 10 of these subjects, shows that the instructions to the subjects eliminated

the 'deliberate,' large-amplitude, initial responses which would otherwise have been included with the smaller, more involuntary conditioned responses. This is evidenced by the initial rapid rise and subsequent fall of the curve.

The development of the conditioned response itself is shown by the lower curve, from which the data of the ten subjects who responded deliberately were excluded. This curve has a positive acceleration in the earlier stages of the conditioning process similar to other curves of the acquisition of a conditioned response (**6, 7, 8, 14, 21**). Note that the lower curve does not begin to level out until the very end of the session. This can be readily explained by the small number of training trials given to prevent maximal conditioning of the eyelid reaction; with further training the curve would have completed its S-shape.

The steeper rise of the second half of the upper curve indicates that the development of the conditioned response for those subjects who responded deliberately at first was facilitated by their earlier 'deliberate' responses. As an association between the conditioned stimulus and the eyelid response had already been established, conditioning proceeded more rapidly.

Tests of the "Inhibition of Delay"

The development of the delayed conditioned response during the training has been discussed elsewhere (**18**). There it was reported that the latency of the conditioned galvanic skin response increased from a mean of 4.8 seconds to a mean of 10.1 seconds during the experimental session. In 74 out of 80 tests of the 'inhibition of delay' (last four test cycles only), the test stimuli for the conditioned lid response fell within the period of delay.

The summary of the mean amplitudes of the conditioned eyelid responses for the last four test cycles is presented in Table 1. It may be seen that there is a weighted mean [5]

[5] The mean values used in this discussion are weighted means based on the variable number of satisfactory test records obtained for each subject. Although each subject was given the same number of tests, some of the records of the conditioned lid reaction had to be discarded because of faulty registration, interference by spontaneous winks,

TABLE 1

SUMMARY OF THE MEAN AMPLITUDE IN MILLIMETERS OF CONDITIONED LID CLOSURE AT EACH OF THE THREE STIMULATIONS OF THE TEST CYCLE

On test trials the test vibrator-puff stimulation was presented during trials 36, 40, 45, and 48 of the persisting light-shock combination, 5 seconds within the period of delay. Pre-test and post-test trials completing each test cycle consisted of conditioned lid reactions to the vibrator-puff combination when it was presented alone at variable intervals before and after the test trials. Negative values signify a decrement of the test conditioned lid reaction.

Subject	Total No. Test Cycles	Pre-test Trials	Test Trials (T)	Post-test Trials	Mean of Pre-test and Post-test (M_{pp})	$T - M_{pp}$	$\frac{T - M_{pp}}{M_{pp}} \times 100\%$
1	4	12.3	14.0	17.3	15.2	−1.2	− 8%
2	4	14.4	11.3	13.1	13.7	−2.4	−18%
3	4	3.3	4.6	7.2	6.3	−1.7	−27%
4	4	15.1	14.0	14.7	14.9	−0.9	− 6%
5	4	14.8	12.1	17.4	16.0	−3.9	−24%
6	4	14.3	13.9	14.6	14.4	−0.5	− 3%
7	4	17.1	17.0	17.0	17.0	0.0	0%
8	4	14.3	11.0	18.3	16.3	−5.3	−33%
9	2	3.4	0.0	9.1	6.3	−6.3	−100%
10	4	20.3	17.3	21.0	20.7	−3.4	−16%
11	3	7.6	8.1	10.2	8.9	−0.8	− 9%
12	4	14.0	10.0	12.7	13.4	−3.4	−25%
13	4	25.1	24.9	25.5	25.3	−0.4	− 2%
14	4	6.3	0.7	9.2	7.7	−7.0	−91%
15	4	16.7	16.0	16.9	16.8	−0.8	− 5%
16	2	13.9	13.9	15.6	14.8	−0.9	− 6%
17	2	16.0	17.8	15.9	16.0	+1.8	+11%
18	2	16.2	15.9	14.7	15.5	+0.4	+ 3%
19	4	13.4	12.6	14.7	14.0	−1.4	−10%
20	4	12.7	10.3	14.7	13.7	−3.4	−25%
Total....71							
Weighted Mean...13.9		12.4	15.2	14.6	−2.2		−19%
Mean							−24%
σ_M							5.4%
$\frac{M}{\sigma_M}$							4.4

decrement in the eyelid reaction of 19 percent on test trials occurring in the interval of delay. This decrement is quite reliable in view of the high critical ratio of 4.4 for the arithmetical mean of − 24 percent and the fact that 17 out of 20

or failure of the recording mechanism. If one of the records constituting a particular test cycle was faulty for any of these reasons, the whole cycle was discarded. However, the standard errors and the critical ratios are based upon straight arithmetical means.

subjects show this tendency. Of the three subjects who manifest no decrement, one does not show any change, and the other two show only small differences in the opposite direction.

The mean latencies presented in Table 2 [6] indicate a

TABLE 2

SUMMARY OF THE MEAN LATENCY IN MILLISECONDS OF THE CONDITIONED LID RESPONSE AT EACH OF THE THREE STIMULATIONS OF THE TEST CYCLE

These values correspond to the mean amplitude measures given in Table 1.

Subject	Total No. Test Cycles	Pre-test Trials	Test Trials (T)	Post-test Trials	Mean of Pre-test and Post-test (M_{pp})	$T - M_{pp}$	$\frac{T - M_{pp}}{M_{pp}} \times 100\%$
1	4	461	439	383	422	+17	+ 4%
2	4	363	471	411	387	+84	+22%
3	4	261	275	240	251	+24	+10%
4	4	334	408	353	343	+65	+19%
5	4	250	251	190	220	+31	+14%
6	4	293	290	275	284	+ 6	+ 2%
7	4	351	321	285	318	+ 3	+ 1%
8	4	259	299	220	239	+60	+25%
9	2	300	500	293	297	+203	+68%
10	4	263	294	243	253	+41	+16%
11	4	331	343	366	349	− 6	− 2%
12	4	276	330	256	266	+64	+24%
13	4	263	281	259	261	+20	+ 8%
14	4	341	473	330	337	+136	+40%
15	4	469	429	453	461	−32	− 7%
16	2	433	453	265	349	+104	+30%
17	2	395	415	380	388	+27	+ 7%
18	3	347	375	422	384	− 9	− 2%
19	4	303	389	285	294	+95	+32%
20	4	311	361	320	316	+45	+14%
Total...	73						
Weighted Mean....		326	364	310	318	+46	+15%
Mean							+16%
σ_M							3.9%
$\frac{M}{\sigma_M}$							4.1

similar tendency in the expected direction. The conditioned wink occurring in the delay period shows a mean increment in latency of 15 percent over the pre-test and post-test controls.

[6] In a few cases no conditioned eyelid response occurred on test trials. These were included in the amplitude measures as zero amplitude. The latency values for these cases were taken arbitrarily as the length of time between the onset of the vibrator and the air-puff.

This difference is 4.1 times its standard error. Only three subjects show a small reversal in the opposite direction.

The similarity of the amplitude and the latency values indicates a significant though not too high negative correlation between the two sets of measures. The product moment correlation between the mean amplitude and the mean latency measures of the several pre-test, test, and post-test conditioned responses for each of the twenty subjects [7] of one of the control experiments was found to be $-.49 \pm .09$. This compares favorably with the correlation of $-.39$ based on mean values found with conditioned responses in dogs by Hilgard and Marquis (7). However, the correlation between individual measures should be much lower, since the use of means obscures some of the variability. For example, the correlation of the measures of one subject who gave 23 conditioned responses was found to be only $-.06$.

In view of the high reliability of both the amplitude and the latency differences, this tendency of the test conditioned response to be partially inhibited when it occurs in the delay period of the delayed conditioned response may in all probability be regarded as genuine.

The results from the control series, however, indicated that the persisting light stimulus tended to inhibit the test response even before any conditioning had occurred, although this tendency was considerably less marked than that observed after the delayed conditioned response had been established. The decrement in amplitude of the test response was 10 percent; the increment in latency, 13 percent. The critical ratios for these values, based on the standard error, are 1.2 and 3.4 respectively. There was, however, a rapid drop in this inhibitory effect from the first to the third test cycle of the control series, which indicated an adaptation of the subject to the persisting visual stimulus.

The possibility then arose that the inhibitory effect obtained in the experimental series was largely a result of the continuation and intensification of that observed in the control series. The greater inhibitory influence of the period of delay might be due not to the conditioning process but to the shock

[7] Each mean comprised a total of three to four eyelid responses.

per se through the arousal of interfering sensitized responses (**2**, **5**, **21**) to the light. The presentation of the shocks alone might conceivably arouse a general apprehension in the subject which would increase the inhibitory effect obtained in the control series. To investigate this possibility, two control experiments with twenty subjects each were performed.

The first group of control subjects was treated exactly the same as the Experimental Group, with the single exception that no shocks whatever were given. The object was to measure the effect of continued adaptation to the light alone upon the test conditioned lid response. The difference between the results of this group and those of the Experimental Group should be due to the presence of the shock in the latter, which would permit a determination of the effect of the shock upon the test response.

The second group of control subjects was also put through the same procedure as the Experimental Group, except that the light and shock stimuli were never presented in a combination which would set up a conditioned reaction. The stimuli were scrambled systematically in such a manner that if any conditioning did occur, the response would be promptly extinguished. The difference between the Experimental Group and this Control Group should permit an estimation of the effect of the shock when unassociated with the visual stimulus.

Comparison of the Experimental Group with the Control Groups

The comparison of the three groups of subjects for both the control and experimental series is shown in Table 3. All the groups were treated identically during the control series, the procedure varying only during the experimental series.

It may be seen in column 2 of Table 3 that while the Experimental Group manifests a decrement of 19 percent in the amplitude of the eyelid response in the tests of the experimental series, the two control groups register either no change (Shock Group) or an actual increment of 9 percent (No-Shock Group). Thus the amplitude of the conditioned wink showed

TABLE 3

SHOWING THE MEAN PERCENT DIFFERENCE IN AMPLITUDE AND IN LATENCY BETWEEN THE MEAN OF THE TEST AND THE MEAN OF THE PRE-TEST AND THE POST-TEST CONDITIONED EYELID RESPONSES FOR EACH SERIES OF EACH GROUP OF 20 SUBJECTS

Negative values signify a decrement of response on test trials. The critical ratios are based upon arithmetical means.

Group		Control Series		Experimental Series		Difference between Exper. and Control Series (3)	Difference between Exper. Group and Control Group (4)
		Weighted Mean (1)	$\frac{M}{\sigma_M}$	Weighted Mean (2)	$\frac{M}{\sigma_M}$		
Experimental	Amplitude.	−10%	1.2	−19%	4.4	−9%	
	Latency...	+13%	3.4	+15%	4.1	+2%	
Shock Control	Amplitude.	−16%	1.9	−1%	0.4	+15%	−24%
	Latency...	+13%	5.0	+7%	2.1	−6%	+8%
No-Shock Control	Amplitude.	−13%	1.1	+9%	1.5	+22%	−31%
	Latency...	+9%	3.7	+3%	1.6	−6%	+8%

an 18-percent greater decrement in the tests of the Experimental Group than in those of the Shock Group. The difference between the arithmetical means of these two groups is 3.10 times the standard error of the difference, which is quite significant. The difference between the experimental group and the No-Shock Group is 28 percent. The relative differences in the latency increments of the test responses may be seen to follow a similar trend. The difference in latency of 8 percent between the Experimental Group and the Shock Group is 1.74 times the standard error, and represents about 95 chances in 100 that the difference is a true one.

Inasmuch as the only difference in procedure between the two control groups is the presence of the shock, the greater inhibitory effect obtained with the Shock Group must be a result of the influence of the faradic stimulation, when it is unassociated with the visual stimulus.

Since the three groups of subjects did not manifest the same amount of change in the test responses during the control series (column 1, Table 3) even though the procedures were identical, the groups may have been unevenly balanced as to the number of subjects exhibiting a marked inhibitory effect.

This would account for the inequality of the means of the inhibitory effect in the control series of the three groups of subjects. Unless these groups were balanced in some way, this same constant error would be operative in a direct comparison of the results of the experimental series. To avoid this possibility, the amount of the inhibitory effect obtained in the test cycles of the control series was deducted from the results of the experimental series. This procedure is equivalent to making the inhibitory effect obtained in the control series of each group of subjects the base line, from which the results of the experimental series deviate.

The values obtained in this way are given in column 3 of Table 3. It may be seen that the Experimental Group manifests a 9-percent greater decrement in the amplitude of the test lid response in the experimental series than in the control series, whereas the No-Shock and the Shock groups show 22 percent and 15 percent less decrement respectively. As shown in the last column of this table, the Experimental Group exhibits 31 percent more decrement in the amplitude of the test response during the experimental series than does the No-Shock Group, and 24 percent more than the Shock Group.

The latency values corresponding to the amplitude differences discussed above indicate that the increment in the latency of the test response is 2 percent greater in the experimental series than in the control series. On the other hand, both control groups show a decline of 6 percent from the increment of the test response latencies of the control series, making a net difference of 8 percent between the Experimental Group and the Control Groups.

Since the presence of the shock when it is not associated with the light only slightly retards the subject's adaptation to the light alone (*cf.* experimental series of both control groups), it seems safe to assume that the inhibitory phenomenon observed with the Experimental Group is somehow a result of the association of the shock with the light.

In order to determine whether there was any progressive change in the amount of decrement of the conditioned lid

response during the experimental series, the mean difference between the test winks and the mean of the pre-test and post-test winks for each test cycle was plotted in Fig. 6. Here the mean decrement in amplitude of the test wink is definitely greater for each of the last four test cycles of the Experimental Group than for either of the Control Groups. The corresponding latency values showed a similar trend.

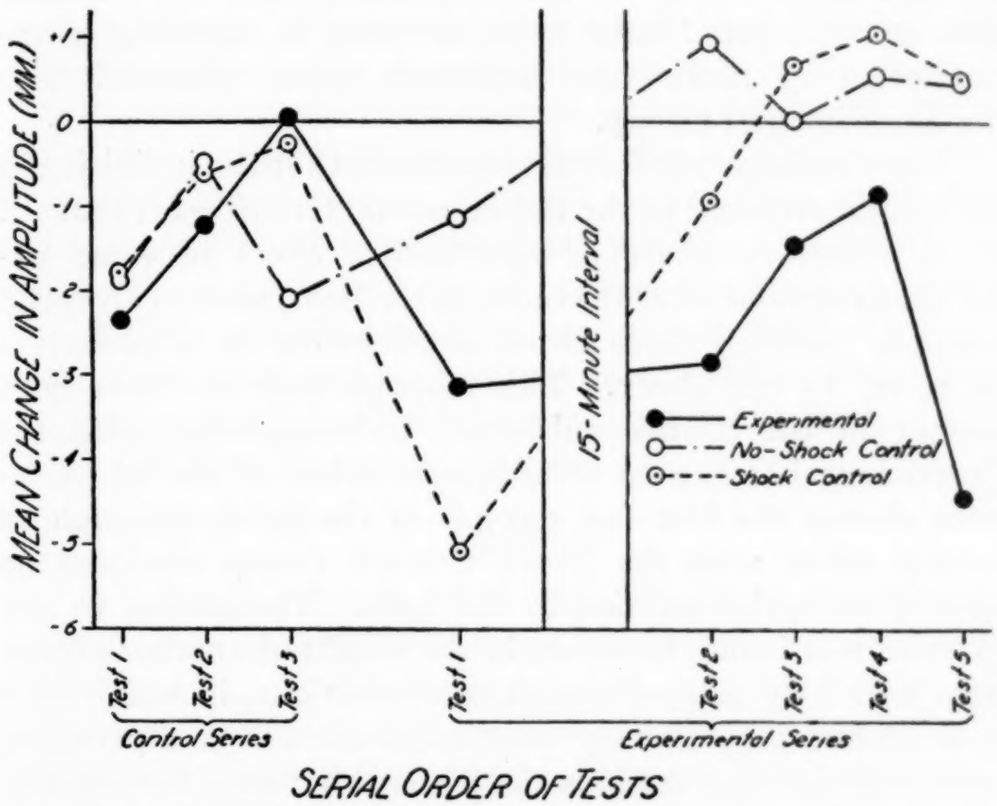

FIG. 6. Showing the mean change in the amplitude of the test conditioned eyelid response from the mean of the pre-test and post-test controls for each successive test cycle of both the control and the experimental series of the three groups of subjects.

It will be observed that there is an appreciable decrement in the amplitude of the test winks of Test 1 of the experimental series (sixteenth presentation of the light stimulus). This decrement is greater for the Experimental Group than for the No-Shock Group. The reversal of the Shock Group must be accepted with reservation, as the difference between this group and the Experimental Group is only .94 times its standard error.

By further inspection of the curve of the Experimental

Group in Fig. 6, a decidedly greater decrement in amplitude is seen on the final test cycle. The test latencies showed more increment on the last two test cycles. Thus there does not seem to be any definite factual basis for assuming a progressive adaptation of the inhibitory effect during the tests of the experimental series, although the possibility remains that it may be disguised by some other factor producing the U-shape in the curve. If this adaptation were very considerable, caution would have to be exercised in ascribing to the 'inhibition of delay' the inhibitory effect observed with the Experimental Group.

There remains the further possibility that the inhibition of the test lid response in the Experimental Group was produced by a refractory phase or interference effect of some unrecorded response after the onset of the light, such as an eyelid reaction, resulting from either conditioning or sensitization (**2**, **5**, **21**) by the shock. This reaction may not have been present in the control subjects. Unfortunately, with the Experimental Group no records were taken of the lid movement during the first few seconds of the light, although 20 records taken with the Shock Control Group disclosed no signs of an eyelid reaction to the light. The answer to this question was found, however, in the results of another experiment with long delayed conditioned reactions, in which by a series of five progressively lengthened steps over a period of seven experimental sessions the interval between the onset of the persisting light stimulus and the shock was increased to 60 seconds. At the end of this period the conditioned lid response was established and tests of the 'inhibition of delay' were made by presenting the stimuli for the lid response 15 seconds after the onset of the light. The data of 11 tests given to 3 subjects indicated that the amplitude of the lid response was inhibited to an extent of 24 percent when it occurred 15 seconds within the period of delay. This had a critical ratio of 4.0 based upon the standard error. The corresponding latency increment was 13 percent.

It thus appears that the inhibitory effect obtained with the Experimental Group is not confined to the onset of the

conditioned stimulus, but may persist for a considerable period of time. The indication is therefore that the interval of delay of a delayed conditioned response possesses inhibitory characteristics which may depress another conditioned reaction occurring during that interval. Corroborative evidence [8] suggests that a similar inhibitory effect is obtained with trace conditioned responses.

Pavlov (**17**, 103) has suggested that this behavioral phenomenon may be reduced to the generalized inhibition supposedly operative in experimental extinction (**1**, **19**). The development of the delay may be accounted for by the fact that the responses near the onset of the conditioned stimulus are extinguished because they are remote from the point of reinforcement. Pavlov's explanation of the inhibition of conditioned responses in terms of cortical processes is, however, much less convincing. The underlying mechanism may eventually be found to be more in line with other psychological and physiological processes operative in the facilitation and inhibition of reaction systems (**22**).

Summary

1. Twenty subjects were employed in an experiment to test for the 'inhibition of delay' in delayed conditioned responses (*i.e.*, to determine whether the interval of delay possesses inhibitory characteristics which may be generalized to other conditioned responses). On the first session of one hour, a conditioned lid response was established to a tactual-vibratory stimulus followed by a puff of air to the cornea. On the second session of two and one-half hours' duration, a delayed conditioned galvanic skin response was set up to a light reinforced after a 20-second interval by an electric shock. On certain trials, the stimuli for the lid response were presented 5 seconds within the interval of delay. The amount of change in the amplitude and latency of the conditioned lid response on these test trials as compared with the magnitude of this same response under normal conditions constituted a measure of the inhibitory characteristics of the interval of delay.

[8] An unpublished experiment by Professor Clark L. Hull.

2. The visual stimulus alone produced a mean decrement of 13 percent in the amplitude and a mean increment of 13 percent in the latency of the test conditioned lid response before any reinforcement of the light by the shock had occurred. There was a rapid decrease of this inhibitory tendency with continued non-reinforcement of the visual stimulus.

3. After a delayed conditioned response had been established, the conditioned lid reaction suffered a reliable decrement of 19 percent in amplitude when it occurred in the interval of delay. There was a corresponding increment of 15 percent in the latency of the response.

4. Two other groups of 20 subjects each were employed in control experiments. The first control group was treated exactly the same as the Experimental Group with the exception that no shocks were ever given. The second control experiment differed only in the respect that the visual and shock stimuli were never presented in a combined order. Neither of these groups showed any decrement in the amplitude of the conditioned wink on test trials. Only a slight increment occurred. Less inhibitory effect was obtained with the first control group than with the second. Thus the inhibition of the eyelid response in the main experiment was definitely a result of the associated order of the visual and shock stimuli.

5. Three subjects were employed in another experiment on long delayed conditioned responses in which the interval between the onset of the visual stimulus and the shock was extended to 60 seconds. A 24-percent decrement in amplitude and a 13-percent increment in latency was obtained when the test lid response occurred 15 seconds within the interval of delay.

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PERCEPTIVE ERRORS IN TIME JUDGMENTS OF BEHAVIOR

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I. Antecedents of the Experiment

The temporal attributes of behavior are a problem which has been of recurring interest since the first experiments on reaction time in the early Wundt laboratory. Attempts to measure the temporal factors in emotional responses, children's play, animal learning, and some of the higher forms of skill have been filled with possibilities of errors in time perception committed by the recorder. It is easy to say that time should be indicated by automatic instruments, but the perception of critical cues, even in calibrated instruments, is a psychological problem not always faced by research workers. For instance, the motion picture camera, as a means of recording behavior phases, may in some situations be no improvement on direct perception; a strip of film has its greatest value as a permanent record for further verification, in many experimental sets.

The following physical and physiological statements are probably true at the present writing, but may not remain true with further developments of instruments and materials. The dark adapted human eye is more sensitive to green light than the most sensitive photoelectric cell commercially available. The retina of the human eye is still capable of transmitting a more detailed image of the external world, *in relation to the area covered*, than the finest grained motion picture film available on the market. (An interesting parallel is the fact that the human nose is many times more sensitive to some chemicals than the very best laboratory reagents.) It is a fact, without question, that differential movement, in still frame analysis of motion picture film, may be obscured by chemical grain in situations where it would be visible if

observed directly. The advent of 'supersensitive' film has not brought the camera, even with high speed lenses, down to the level of the human eye in making observations under unfavorable light conditions. (These are important facts even if we do not consider the matter of expense as an item.) Of course, the motion picture camera is a very important recording instrument in the taking of psychological data, when its limitations are known, and it would be ridiculous to say that direct perception should always be used in preference to a camera. There is no small amount of urgency in the fact that film records provide very useful verification possibilities, and give judgments on behavior completely free from some of the perceptive illusions in a biased experimenter. And we might comment, perhaps with sophistication, that all experimenters are biased.

But just as we standardize the amount and type of errors in mechanical instruments, so should we know the error characteristics committed by human observers. What are the characteristics of a situation in which time observations are made with the least error? What are the effects of admitting conditions of error, measured as accurately as possible on progressive scales? Certainly the psychologist, as a recorder of scientific data, should know himself.

The problem of optimum conditions for correct time judgments, by the reaction method, has been anticipated, either by direct intention or incidentally, by many workers since the end of the eighteenth century. The topic was certainly thrust into psychological literature by the famous Greenwich Observatory case in 1796, when an assistant, Kinnebrook, lost his position because his observations of the time at which a star crossed a transit line did not agree with the reports of Maskelyne, his superior. That assistant became somewhat of a martyr in establishing what is now known as the 'human equation' among most workers in the physical sciences, but is known as "reaction time error" in the psychological laboratory. It was almost a century later that Cattell and his associates added to psychological literature their discoveries concerning reaction-time characteristics.

In using a key-controlled instrument, the errors of reaction time will be an important component of the errors of time recording. If all conditions are favorable, and there is a sharp and instantaneous critical cue for the beginning and end of the behavior phase, the probable error for making time responses, for phases long enough to make possible two separate responses, should be no greater than twice the probable error of simple reaction time. This is based on the logical assumption that variable errors of initial and terminal responses are as likely to be summational as compensative. To the extent that conditions are more favorable than the simple reaction time situation, even this probable error may be reduced.

The conditions which increase the size of errors in time judgments, involving reaction-time characteristics, have been suggested in several researches. The importance of muscular set in making accurate rhythm responses was noted by R. H. Seashore (**9**). R. Sollier (**11**) found that it was more difficult to stop a muscular process than to start it, on the presentation of a cue. For purposes of accurate timing, this suggests that the two responses used in signalling the beginning and end of a behavior phase should be as nearly identical as possible, or should be equated for reaction time error, or should be calibrated for their reaction time differential. H. Piéron, (**6**) working with reaction time to the cessation of a visual stimulus, held that latent period under these conditions, as with reactions to the beginning of a visual stimulus, decreases as the illumination intensity increases, and that the relationship is one of a hyperbolic law. H. V. Gaskill (**3**) found that shorter reaction times are secured if the stimulus occurs during the crest of a breathing cycle. The importance of critical discriminative cues is suggested by the work of F. W. Irwin (**4**) on indices of facial expression. B. Sandor (**8**) came very close to the problem of this paper, in one aspect, when he held that the second of two reactions becomes more delayed as it impinges closer to the first. He verified an old and well-known statement that adequate forewarning shortens the reaction time.

The conditions for the so-called complication experiments, probably started by R. Dodge (1), and since followed by K. Dunlap (2) and others certainly bear on the description of conditions for error in time recording by the reaction method. Dunlap noted that differences in the position of the moving object, as perceived by the subject in relation to the background, have been secured dependent on whether the observer kept his eyes continuously on the moving object, or continuously on the scale point, or shifting from one to the other. If the subject fixated the pointer habitually, the sound of the bell (critical stimulus) was followed by a tendency to report the moving object as prior to its correct position. If he fixated the scale, the moving object was habitually reported as later than its correct position. Mixed tendencies produced variable results. From the standpoint of accurate time recording, it would appear that a subject's consistency in fixation habits is the most important item from these experiments.

The neglect of the importance of discriminative perceptual cues, in recording both the temporal and pattern characteristics of behavior, was at the bottom of the exposure by Mandel Sherman (10) of Watson's possible errors in classifying the emotions of infants. Not wishing to trust all of the discriminative abilities of an 'observer' in the introspectionistic sense of the word, it appears that a behaviorist was caught backstepping into a pit of discriminative errors in the experimenter. This, of course, in no way removes the fallacy of the ambiguity of language found in an introspectionist's use of an 'observer's introspections,' and in no way reduces the value of the cross check possible in behavioristic approaches. Rather, Sherman's research was a good example of the type of check possible only in a regime where objective data is taken as the sole basis of generalization. But we are most certainly led to the importance of locating the sensory cues before we start making time judgments.

The research of E. K. Rumberger (7) purported to furnish direct evidence on the problem of accurate time recording by the use of the stop watch. Why a person, in the present state

of development of refined instruments, should limit himself to the use of a stop watch calibrated in tenths of a second, we do not know. As a criticism of the habits of timers in athletic events, such a piece of work could reach its highest significance only when compared with a more accurate instrument. Under optimum conditions the minimum calibration of such a stop watch, a tenth of a second, is five times the absolute error of a skilled timer using a chronoscope, when making time judgments of a moving human arm, and using groups of ten judgments for establishing the median measurement. The reader is referred to Tables VIII and IX of this article. Further, the use of a red signal, appearing instantaneously twice, and disappearing between exposures, is not a parallel of the conditions found in timing human or animal behavior phases, though it does somewhat approach the typical simple reaction time situation. It is exceedingly mportant to remember that in responding to the beginning and end of a behavior phase the moving object is *continuously visible* and can be seen approaching the critical position used as a basis for making the time signals in such a manner as to constitute a forewarning more adequate than the 'ready' signal given a subject in a reaction time experiment. We cannot believe other than that Rumberger's data were heavily biased by mechanical errors, and that the conditions of his experiment were so unfavorable that a stopwatch calibrated in tenths of seconds showed some sort of statistical reduction of errors for skilled timers.

There is a possibility that our final objections to Rumberger's work also apply to the research of A. M. Loomis (**5**) on an even larger scale. She tabulated the variations of observers estimating the duration of time intervals from behavior cycles exhibited in motion pictures. So little is given of the exact conditions of her experiment, and the methods of making the time responses, that it is difficult to make any decision concerning the adequacy of her controls. In general she certainly does not seem to have any intention of starting with time judgments under the most favorable conditions of accuracy, and pursuing the causes of error as separate vari-

ables. One gets a general impression of the wide variability of subjects, not expressed quantitatively, and that some situations are more inaccurately reported than others. She says, "The more frequent the record of transitions from one kind of behavior to another, *i.e.*, the more intermittent the activity of different sorts, the greater on the average was the observational error. This is a difficulty in timing techniques." Later in the article, "But we have not ventured to allocate precise degrees of error among observers or to specific situations." The use of 5-second units in displaying subject variability seems to indicate that the conditions were vastly more difficult than those used in the present research, and were probably complicated by so many causes of error that functional statements were impossible.

II. Apparatus

It was our object to expose the typical errors of the reaction method of making time records in such a manner that the subject's judgments, on the reaction keys, could be compared with a very accurate objective criterion. In no case did we allow the mechanical probable error to exceed one millisecond.[1] The local alternating current was used for operating the synchronous chronoscope only after securing frequency charts of the afternoon output of current from the power distributor who happens to be a member of an extensive organization of synchronized power distributors who have achieved rather accurate control of cycle frequency. The probable error of frequency variation, for a period of a half second, is considerably less than one millisecond. The 'hunting' characteristics of synchronous motors is a more potent cause of error than the possible irregularities of frequency in such a commercial source.

All chronoscopes were tested by the fall apparatus and adjusted until the probable error was less than one millisecond. It was found almost impossible to keep the most common make of Dunlap instrument to such a degree of accuracy. The two clutch plates persist in developing unequal friction characteristics, and the needle shaft is so heavy that its load, suddenly thrown on or off the motor, induces violent hunting tendencies in the rotating armature. We built our own chronoscopes, for some of this experiment and for other laboratory purposes, with light shafts and needles, and with 'dog-tooth' clutches which act positively and are not dependent on a changing coefficient of friction.

In the experiment described in Section IV the stimulus was a rotating pointer driven by a synchronous motor, passing around a dial on which were placed black, wedgeshaped points so located that the needle consumed exactly 2 seconds in passing from one critical mark to the next. The point of the needle travelled at such a speed

[1] The term *millisecond* for a unit equivalent to .001 second will be used throughout this article in preference to the term *sigma*, which has become confused with other references than that of time, particularly as a symbol of a certain method of determining statistical variability.

that it consumed 1 millisecond of time in passing 1 minute of the visual angle. The subject responded on two matched keys, of the break variety, one for starting the chronoscope as the needle passed the first critical point, the other for stopping the chronoscope as the needle passed the second critical point. Between the two points the subject was required to move his hand laterally perhaps 10 centimeters to change from the starting key to the stopping key. In this and all further experiments the background was always made from a white glazed paper reflecting 30 percent of the light, and the pointer was painted black, with a reflection ratio 1/32 of the background. Illumination at 50 footcandles of tungsten light was so arranged that lamps were not in the line of vision of the subject. Light measurements were made by means of a Weston Illuminometer of the photoelectric cell variety.

In the experiment on the relation of time errors to the interval being judged, Section V, a relay circuit was devised in such a manner that the same key, in the hands of the subject, was used to start the chronoscope, at the beginning of the behavior phase, and to stop the chronoscope at the termination of the behavior phase, thus making the two responses as nearly identical as possible with respect to the neuro-muscular characteristics. The circuit diagram is pictured in Fig. 1, and is essentially

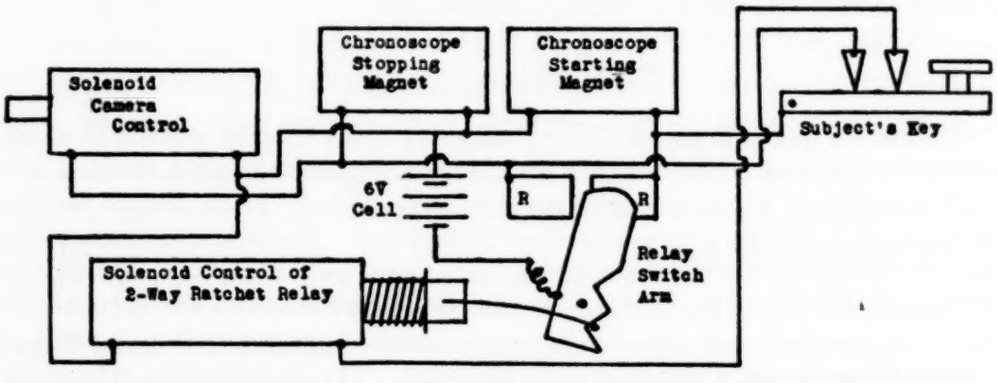

FIG. 1. Chronoscope relay circuit for starting and stopping the chronoscope by means of the same key. *R, R*, contact plates for relay switch arm.

a modification of the usual electrical controls of the Dunlap chronoscope, the needle being started by deënergizing one magnet, and stopped by deënergizing an opposed magnet. Ordinarily a stimulus key is placed in the starting circuit and a reaction key placed in the stopping circuit. In this modified circuit it will be obvious that the relay switch arm changes the subject's key from the starting magnet to the stopping magnet, and *vice versa*, in such a manner that the subject makes the same type of response at beginning and end of the behavior phase. The breaking of the circuits operates the chronoscope instantly, but the relay does not complete its switching operation until the circuits are reëstablished at the moment of raising the hand. A solenoid control of the camera, attached in shunt circuit to the stopping magnet, operates in such a manner that a picture is taken of the dial setting each time the needle stops.

Though the above improvement was made to keep constant the nature of the reaction made, it actually made but little difference in the final results, the matched keys were so nearly alike in the neuro-muscular characteristics needed for operation as shown in the tables which are to follow. In this section of the experiment the needle, moving at the rate of one minute of the visual angle per millisecond of time, travelled on an endless chain pulled by a synchronous motor and sprocket wheel, in such a manner that all movement was in the same direction on the retina. It was expected

that this would eliminate a variable present in the use of the rotary pointer. Wedge shaped critical points were mounted in such a manner that time intervals could be varied from .1 second to 2 seconds.

In the experiment on the relation of error to the speed of the moving object on the retina, Section VI, intervals of 10 seconds were used throughout. Again the single key relay circuit was used. This time we returned to the rotary pointer as a moving stimulus-object, the point of which travelled the orbit in exactly 10 seconds. The subject gave the first reaction when the needle passed the critical point on the lower side of the dial, moving in a left-right direction. The subject gave the second response, stopping the chronoscope, when this pointer passed the same critical position a second time. Stimulus conditions for beginning and end of behavior phase were, therefore, absolutely constant within the probable error of the synchronous motor. Variations in the size of the visual angle, in relation to the constant speed of the pointer, were introduced by means of a plate glass mirror into which the subject gazed as he reacted to the image of the moving pointer. The mirror was moved farther from or nearer to the subject, establishing such positions, by computation, that they gave the size of the visual angle desired without disturbing the mechanical characteristics of the set.

The final experiment, Section VII, tested the size of errors in reacting to time phases of behavior for a human being, conditions being such that an approach was made to the optimum requirements as determined in the preceding work. The first portion of this experiment was accomplished by the use of two chronoscopes, one for measuring the actual time consumed by the moving human, the other to measure the accuracy of the subject acting as time recorder. The stimulus situation made use of two double throw switches clamped to a table, separated by a distance of three feet. One of the switches started a chronoscope and the other stopped it. The moving human target, always starting from a distance several feet away, broke the first switch by a sweeping arm movement, certainly in excess of one minute of the visual angle for 10 milliseconds, made several irregular passes as his own discretion, and then broke the second switch by the same sweeping motion. The time recorder, or subject, was told to react, at the moment each switch was broken, on matched reaction keys, operating the second chronoscope. In another portion of this experiment we used identical conditions except that magnet reaction pointers on smoked kymograph paper were substituted for the chronoscopes. This was done at a time when we were having unusual difficulty in keeping the probable error of the chronoscopes to less than one millisecond.

In experiments of Sections V and VI all chronoscope dial readings were photographed in miniature on 16 mm. film, one exposure of a dial-setting to each frame. All readings were checked for error by examining the enlarged projection image of this film. A permanent, objective record was thus left, in the order the readings were taken, and the experimenter did not know the trends of the results, in each phase of the experiment, until after the films had been processed, though he did believe that some sort of functional curve would result.

III. Subjects in the Experiment

Although it is not the intention of this article to deal specifically with individual differences, it will be obvious that some of the results are dependent on the specific tendencies of the particular subject used. No generalization from a composite is shown without also indicating the character of the data by individuals. The experiment was influenced by the degree of motivation, it seemed, and therefore the rewards given for acting as subject are indicated. In some subjects the general attitudes appeared to be

better than in others. Scholarship standing is estimated only as a possible clue for conscientiousness. In this kind of an experiment it is probable that training in psychology would have little or nothing to do with results other than to influence motivation. Except for the author of the article, who also acted as subject, all individuals were undergraduates. One was given class credit in experimental psychology as a reward for services; others were given financial remuneration on the rolls of the National Youth Administration (NYA) on an hourly basis. In all tables of data secured from the author acting as subject, the index is FO. The undergraduate subjects are described in Table I.

TABLE I

CHARACTERISTICS OF SUBJECTS OTHER THAN THE AUTHOR

Index	Year and Department	Scholarship	Courses in Psychology	Remuneration and Attitude
DU	Sophomore Ch.E.	High	None	NYA, good
BU	Freshman E.E.	High	None	NYA, good
PO	Senior L.A.	Average	Major	Class Credit, good
SE	Junior Bus. Ad.	Average	6 hrs.	NYA, good
HE	Sophomore Bus. Ad.	Average	3 hrs.	NYA, good
SC	Sophomore Bus. Ad.	Average	None	NYA, good
SM	Sophomore Bus. Ad.	Average	None	NYA, good
WI	Sophomore Bus. Ad.	Average	None	NYA, poor (?)
CR	Sophomore Bus. Ad.	Average	9 hrs.	NYA, good
DE	Freshman Bus. Ad.	Average	None	NYA, good
AP	Freshman Bus. Ad.	Average	None	NYA, good

Abbreviations: Ch.E., Chemical Engineering; E.E., Electrical Engineering; L.A., Liberal Arts; Bus. Ad., Business Administration.

Due to the necessity of doing the work at times which did not conflict with classes, readings were all taken between the hours of 1 and 5 o'clock in the afternoon. All had normal vision or wore glasses which gave excellent correction as reported by an oculist. Subject DU wore glasses for myopia and astigmatism, and there was some doubt concerning his constancy of visual acuity in the experiment involving the use of the mirror in distant positions.

IV. PRELIMINARY EXPERIMENTS [2]

It was the intention to make an attempt to set up conditions such that the time judgments on a moving object, passing certain critical points, should reach *an optimum of accuracy*. It is obvious that some random experimentation was necessary in order to locate some of the best conditions. Some of the optimum factors could be determined by reference to the literature on reaction time and visual discrimination. We chose 50 foot candles of general illumination to be well over the minimum for the best fine line discrimination. On

[2] We wish to thank Mr. Cletus V. Polk for his valuable assistance in taking data described in Sections IV and VII of this article.

the basis of preliminary trials we chose a speed of the object at one minute of the visual angle for one millisecond of time, though we found later this was slightly more rapid than necessary. As indicated in the paragraph on apparatus, we had good contrast at the critical points for responses. Visual cues were equally discriminable at beginning and end of behavior phase. The type of response made was nearly identical for beginning and end of behavior phase. The time interval, in this section of the experiment, was not varied, and the subject used rhythm if he wished.

In this section of the experiment we are not assuming that the conditions are comparable to those involved in judgments on actual human or animal behavior. We are quite willing to admit that the results of this particular set, in themselves, tell us nothing concerning the expectancy of errors in natural behavior conditions. The aim is solely to build an approximation of ideal conditions for making correct judgments of the time duration of phases of movement. Human or animal behavior differs from this model essentially by the introduction of a galaxy of conditions which have a probability of increasing the size of errors made by the reaction method. It is our contention that these causes of increased errors should be studied, one at a time so far as possible, each as an independent variable, until we come closer and closer to the situation of making time records in natural situations. Certainly it is only by such an approach as this that a psychologist, or other scientist, can survey the time record needs of a research situation and make good estimations of the possible sources of error.

It was assumed that the capacity to make accurate time responses would be to some extent a function of training, and that the results of practice would be facilitated if a subject were told the size of his errors continuously. Table II shows the values of the absolute error and the probable error by groups of 10 for 100 readings.[3] Each time a subject responded

[3] In this article probable error is taken as a plus or minus one half the interquartile range. Absolute error is defined as the algebraic difference between the median of a subject's distribution and the true time. Fractions of less than a half millisecond are dropped. Fractions of more than a half millisecond are considered as full units. It

TABLE II

TRAINING SERIES

Errors in milliseconds for subject FO, on 2-second intervals, when he is told each time the size of his error. Absolute error for each group of 10 judgments. Stimulus: rotating pointer. The column under No. indicates the number of the reading in the trial series.

No.	e	No.	e	No.	e	No.	e	No.	e
1	3	21	10	41	72	61	−13	81	106
2	11	22	24	42	−22	62	−15	82	86
3	22	23	−45	43	−11	63	23	83	6
4	−18	24	−65	44	−18	64	17	84	46
5	−12	25	−45	45	−54	65	3	85	15
6	49	26	−16	46	−24	66	−43	86	37
7	37	27	−7	47	−36	67	62	87	13
8	32	28	−61	48	32	68	26	88	22
9	−20	29	−16	49	−19	69	−62	89	34
10	−9	30	−26	50	3	70	3	90	−58
E	7	E	−20	E	−18	E	3	E	28
11	−9	31	−14	51	18	71	16	91	7
12	58	32	−32	52	5	72	67	92	−2
13	−72	33	−3	53	6	73	6	93	−26
14	41	34	26	54	23	74	36	94	19
15	41	35	−8	55	3	75	14	95	17
16	−23	36	19	56	54	76	−39	96	40
17	−41	37	35	57	15	77	−25	97	42
18	−30	38	−89	58	−51	78	8	98	1
19	−6	39	−74	59	−110	79	2	99	1
20	99	40	47	60	−3	80	18	100	7
E	−16	E	−6	E	6	E	11	E	7

Probable Error of entire table, 21.5; Absolute Error, +4.

at the close of an interval he was told the size of his error. Every subject in all experiments was given a training series similar to this one, but the tables of all but one training series are omitted because they do not contribute to the principles which are the goal of this article. In general, the training series had its most important use in correcting persistent errors in some subjects, including the author.

was conspicuous, at an early stage of this experiment, that probable error, as defined, was a better measure of dispersion than standard deviation. Due to the fact that almost every subject displayed, at rather rare intervals, conspicuous failures to make good mental sets prior to the appearance of critical stimuli, there appear extreme cases on the ends of the distribution which are not representative of the subject's ability. Probable error, as a measure of dispersion, and the median, as a measure of central tendency, eliminate the influence of these exceptional cases. The proof of the validity of this statistical treatment lies in the fact that the median of the great majority of all subjects' distributions constitutes the closest approximation of the true time.

Table III gives a series of 100 judgments by the same subject when he is kept in ignorance of the size of his errors. Probable error is increased from 21.5 milliseconds to 30. Persistent errors increase the absolute error from + 4 to + 27. For sake of comparison to motion picture still-frame

TABLE III

TEST SERIES

Errors in milliseconds for subject FO, on 2-second intervals, when he is kept in ignorance of the size of his errors. Absolute error for each group of 10 judgments. Stimulus: rotating pointer.

No.	*e*	No.	*e*	No.	*e*	No.	*e*	No.	*e*
1	−55	21	88	41	47	61	18	81	62
2	3	22	22	42	47	62	28	82	−33
3	8	23	−23	43	38	63	−15	83	82
4	6	24	−31	44	18	64	11	84	−47
5	44	25	50	45	20	65	77	85	93
6	148	26	34	46	−23	66	−56	86	−25
7	31	27	25	47	32	67	45	87	−4
8	37	28	66	48	130	68	64	88	38
9	57	29	57	49	84	69	45	89	1
10	33	30	1	50	−43	70	5	90	−4
E	32	E	30	E	35	E	20	E	−2
11	6	31	52	51	−8	71	−15	91	77
12	−16	32	−28	52	−6	72	48	92	82
13	73	33	94	53	43	73	−15	93	−10
14	60	34	2	54	36	74	72	94	5
15	10	35	43	55	−40	75	19	95	0
16	60	36	43	56	−9	76	48	96	−8
17	44	37	61	57	−23	77	−35	97	−25
18	52	38	52	58	69	78	−32	98	81
19	61	39	31	59	−20	79	−17	99	27
20	8	40	−14	60	−2	80	49	100	−35
E	48	E	43	E	−7	E	2	E	3

Probable Error of entire table, 30; Absolute Error, +27.

analysis of film, it is to be noted that this probable error is less than one half the duration of a single motion picture frame at standard speed. By groups of ten, there is not a single absolute error as great as the duration of a frame at standard speed. The persistent errors of this subject were not displayed by some of the other subjects. Table IV shows the results under the same conditions for subject PO, whose probable error was higher, but whose absolute error was only − 2 milliseconds for 100 readings.

TABLE IV

TEST SERIES

Errors in milliseconds for subject PO, on 2-second intervals, when he is kept in ignorance of the size of his errors. Absolute error for each group of 10 judgments. Stimulus: rotating pointer.

No.	e	No.	e	No.	e	No.	e	No.	e
1	79	21	37	41	−57	61	51	81	−25
2	0	22	−22	42	28	62	−42	82	−35
3	43	23	9	43	41	63	−27	83	−27
4	−101	24	87	44	1	64	16	84	−6
5	8	25	83	45	−22	65	−30	85	−50
6	−21	26	18	46	60	66	−1	86	37
7	88	27	−76	47	−55	67	−77	87	0
8	65	28	−64	48	−99	68	25	88	53
9	−13	29	−34	49	30	69	21	89	40
10	62	30	−82	50	−58	70	−5	90	−65
E	26	E	−7	E	−11	E	−3	E	−16
11	51	31	103	51	58	71	6	91	−108
12	32	32	−24	52	−18	72	−24	92	37
13	49	33	−44	53	−110	73	−135	93	−25
14	42	34	36	54	−46	74	94	94	−84
15	100	35	31	55	−12	75	−16	95	−10
16	−48	36	−35	56	−15	76	12	96	0
17	−107	37	31	57	−4	77	15	97	88
18	−29	38	17	58	−47	78	0	98	39
19	67	39	−2	59	−29	79	14	99	0
20	53	40	−45	60	−15	80	−2	100	−14
E	46	E	8	E	−17	E	3	E	−5

Probable Error for entire table, 34.5; Absolute Error, −2.

V. Errors in Relation to the Length of the Behavior Phase Judged

In order to make stimulus conditions more nearly constant for both beginning and end of the behavior phase, we now introduced the horizontally moving pointers. In order to keep the mode of response constant we introduced the relay control of the response key. It had been our intention to use 10 different intervals, beginning with .1 second and proceeding by increases of 200 milliseconds to and including 1.9 seconds. When the scale was made and tested computationally it was found that the intervals were actually 102, 306, 509, 713, 917, 1121, 1325, 1528, 1732 and 1936 milliseconds, approximately an arithmetical progression. The distance between any two critical markers could be established

merely by lowering two appropriate black, wedge shaped pieces to establish any one of the above steps.

Each subject was given between 200 and 230 readings at one sitting. The scale settings were so arranged, with respect to the order of presentation, that the same phase interval was never used twice in succession, thus discouraging the use of rhythm. Also the readings were so arranged in point of order that the influences of fatigue and training were equated for all period values.

The quantitative results are given in Table V, and the

TABLE V

PROBABLE ERROR AND ABSOLUTE ERROR, EXPRESSED IN MILLISECONDS, IN RELATION TO THE LENGTH OF TIME INTERVALS BEING JUDGED

Stimulus: chain driven pointer.

Subject DE

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	20	20	20	20	20	20	20	20	20	20
P.E.....	16	50	20	39	46	32	27	43	36	26
E.....	+44	+11	−14	−14	−48	−46	−26	−3	−12	+22

Subject BU

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	22	20	22	22	22	22	22	21	22	22
P.E.....	30	80	30	54	36	39	23	47	53	23
E.....	+29	−14	−3	−3	−9	−4	+12	+8	+8	+35

Subject WI

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	21	21	21	21	21	21	21	21	21	22
P.E.....	21	30	42	25	42	47	28	33	19	27
E.....	+103	+9	−4	+8	−12	−23	−5	−30	−20	+6

Subject SM

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	21	21	21	21	21	21	21	21	20	21
P.E.....	15	47	34	35	23	34	41	42	48	35
E.....	+30	−47	−13	−32	−15	−21	−24	−23	−55	−48

TABLE V—*Continued.*

Subject SC

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	20	21	21	21	21	21	21	21	21	21
P.E.....	11	67	45	30	47	38	49	36	35	48
E.....	+34	−15	−38	+8	+17	−39	−5	−28	−14	−15

Subject DU

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	22	22	22	22	22	22	22	22	22	22
P.E.....	20	20	27	35	40	40	34	33	27	35
E.....	+11	−8	−2	+31	+31	−17	+5	−17	+4	−6

Subject HE

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	20	21	21	21	21	21	21	21	21	21
P.E.....	20	78	25	63	41	37	26	50	54	40
E.....	+49	+39	−29	−20	+24	+1	+14	+22	+28	+11

Subject CR

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	9	10	10	10	10	10	10	10	10	10
P.E.....	17	34	26	12	20	31	28	27	6	19
E.....	+9	−72	−29	−27	−20	+5	+38	+28	+25	+9

Composite of 8 subjects

Interval	102	306	509	713	917	1121	1325	1528	1732	1936
Readings..	155	154	157	155	158	158	156	154	156	159
P.E.....	21	55	38	40	40	38	33	38	37	21
E.....	+40	−10	−13	−2	0	−20	+2	−10	−3	0

graphical representation in Figs. 2 and 3. As indicated by the curve from the composite of all readings the following facts seem to be established:

1. Absolute errors do not appreciably increase until the phase interval is less than .7 second. Individual variations from this statement are not demonstrably better than pure chance.

2. Between .7 and .3 second there is a conspicuous tendency for most subjects to 'overcompensate' for the fact that they are going to be hurried in adjusting to the response at the

end of the phase period. This is the cause of the abundance of negative errors, and is further established by the verbal reports of the subjects. This tendency must stand as a further qualification of the work of Sandor (8).

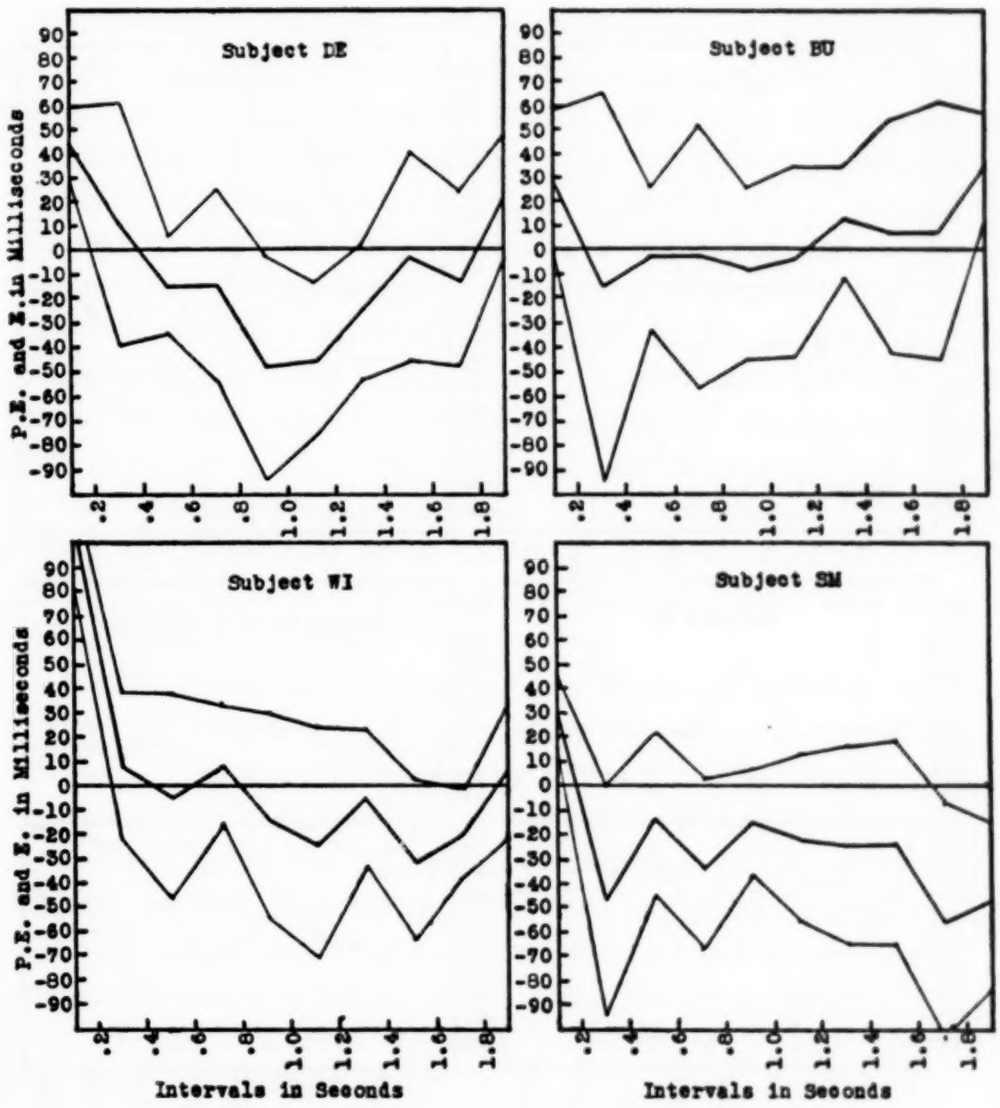

FIG. 2. Data from Table V plotted for subjects DE, BU, WI and SM. Probable error and absolute error in relation to length of the interval being judged. The heavy center line of the curve represents absolute error; the finer lines on each side show the limits of probable error.

3. The period between .7 and .3 second is also accompanied by a definite increase in the size of the probable error, an indication that the subject is having increased difficulty with something new in the conditions for his responses. Verbal

reports from subjects, stated in naïve terminology, were to the effect that the responses to long periods could be anticipated as two completely separated mental events: "Now I *think* of my first response; now I *think* of my second." Subjects, on seeing the 306 millisecond period soon found that they

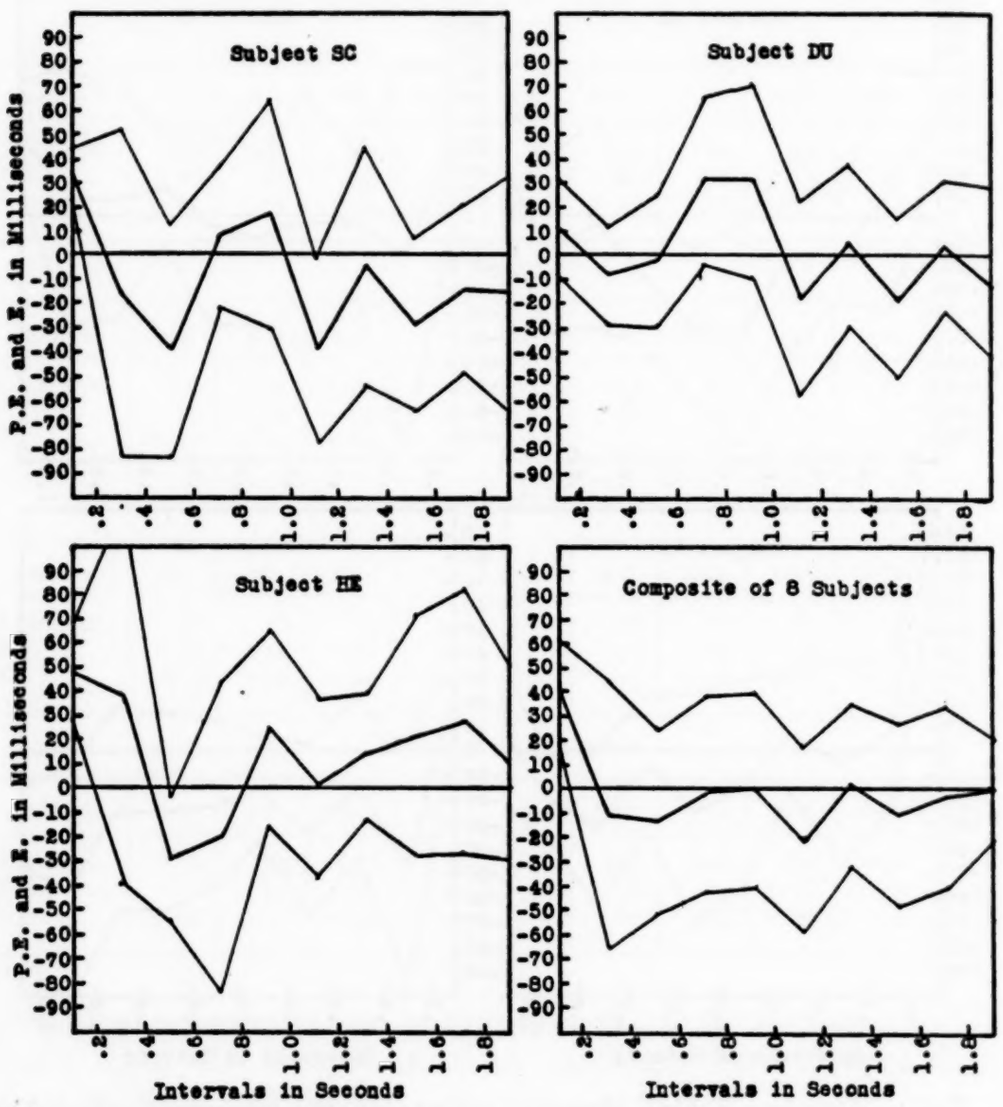

FIG. 3. Data from Table V plotted for subjects SC, DU, HE and the composite of all subjects combined. See Fig. 2 for significance of lines.

could not make a separate adjustment to the end of the phase. Most of them adopted the expedient of getting set for a *double response*, from a single postural adjustment, and attempting to match the adequacy of the time by judging the auditory click of the key in relation to the visual appear-

ance of the pointer passing the critical positions. Some subjects persisted in attempting to make two separate voluntary responses. It is not unexpected that the struggle to adapt produced a higher probable error, especially for the 306 millisecond period.

4. Without exception, the absolute errors of all subjects are heavily positive for the 102 millisecond period. Without exception the probable errors of all subjects at this stage are the smallest of the entire table. The explanation is undoubtedly identical with the conclusions of Sandor (**8**). There is no attempt on the part of any subject to make the two responses the result of two postural adjustments. Each subject merely makes a double response, as rapidly as his powers permit. The stimulus period is definitely shorter than the minimum period between two reactions of any type whatever on the kind of a key used. The small probable error is due sheerly to the stereotyping of simple habit responses. We have passed the 'breaking point' for even a remote approach to a temporal judgment of behavior phases which in any sense is a measurement of time.

5. Phases of behavior probably show no reliable increase in error for longer periods, providing the object is continuously visible. This conclusion seems warranted on the basis of a comparison of the data of this experiment with that exhibited in Table VI for the following experiment. The only qualification on this statement, logically, lies in the possibility of reaching some sort of a fatigue limit, or the introduction of undue attention strains, which need not be a large source of error if the observer utilizes intervening periods between responses for relaxation.

VI. Errors in Relation to the Speed of the Moving Object

We now adopted the rotary pointer stimulus with the 10-second period, and varied the distance of the mirror used by the subject in making his observations. Time was held constant, but the different distances changed in an anti-fatigue and anti-training order. As indicated in the table

the tip of the pointer moved at such a speed that it passed one minute of the visual angle in 1, 10, 15, 20, 25, 30, and 34 millisecond settings. Subjects DU, BU, and AP were young men in their early twenties and should have had no difficulty making the visual adjustments to changes in distance, though DU shows a much higher amount of error for equivalent

TABLE VI

PROBABLE ERROR, EXPRESSED IN MILLISECONDS, FOR JUDGMENTS ON 10-SECOND INTERVALS, IN RELATION TO THE NUMBER OF MILLISECONDS CONSUMED BY THE MOVING POINTER IN TRAVERSING A DISTANCE EQUAL TO ONE MINUTE OF THE VISUAL ANGLE (RATE OF MOVEMENT ON THE RETINA)

Stimulus: rotating pointer

Subject FO							
Rate	1	10	15	20	25	30	34
Readings	30	30	33	32	31	33	29
P.E.	35	21	25	36	32	50	41
Subject AP							
Rate	1	10	15	20	25	30	34
Readings	25	25	24	23	24	23	25
P.E.	27	26	40	50	39	59	61
Subject BU							
Rate	1	10	15	20	25	30	34
Readings	25	25	27	25	26	26	25
P.E.	32	31	46	46	57	59	63
Subject DU							
Rate	1	10	15	20	25	30	34
Readings	26	0	0	10	24	22	22
P.E.	47			59	60	87	106

distances. Subject FO used a + 1.25 diopter correction on the 1 millisecond setting of the mirror. His lower probable error can be explained entirely in terms of a longer previous training, however. Subject DU, ordinarily a very conscientious man, was worrying over catching a train to go home on a vacation and may have been unable to avoid a certain amount of lack of concentration on the work. Only 5 intervals were used on DU and these are not consistent with his results in

other tables, though they show the same trends with respect to general contours.

Table VI gives the statistical values and Fig. 4 shows the graphical representation. Probable error only is given. Absolute error was not significant, nor would it be expected to show anything. The following conclusions seem justified:

1. The optimum speed of the object is in the neighborhood of 10 milliseconds of time per minute of the visual angle, but

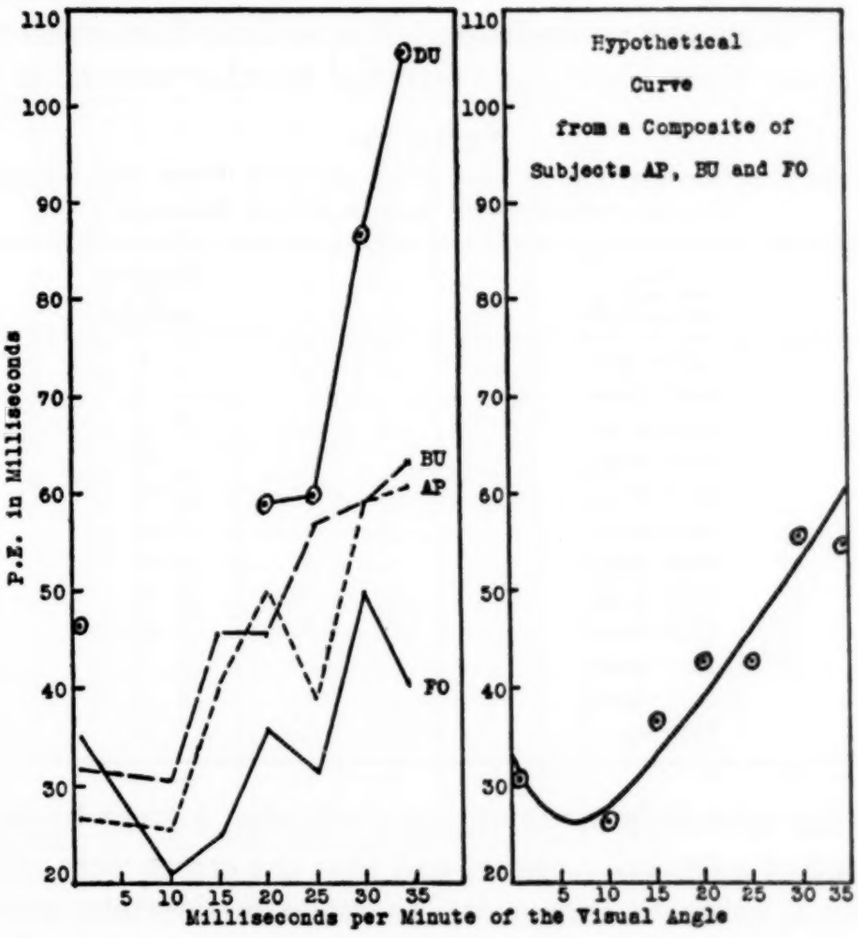

FIG. 4. Probable error in relation to speed of the moving object on the retina for 10-second phases.

not as rapid as one millisecond. We do not believe that the points are sufficiently well established to attempt a mathematical formula.

2. Trends at the ends of the curve suggest that errors may increase for both lower and higher speeds, but particularly for the latter.

VII. Time Reactions on Phases of Human Behavior

The physical conditions for this experiment, as described in the section on apparatus, were such that the forewarning for each response was absolutely dependent on cues obtained by watching the arm of the moving man. It is perfectly true that the actual critical stimulus for the response was the breaking of the visible switch points, *seen in relation to the position of the moving hand.* Thus a sharply defined boundary was provided the subject for what constituted a behavior phase. It must be remembered that we have been contending that some of the high errors reported in other researches have

TABLE VII

A Sample of a Distribution of Time Intervals Used When the Subject Was Making Judgments on Actual Human Behavior

Time Intervals in Milliseconds	Frequency of Stimulus
950– 999	1
1000–1049	1
1050–1099	7
1100–1149	19
1150–1199	16
1200–1249	19
1250–1299	24
1300–1349	18
1350–1399	14
1400–1449	6
1450–1499	2
1500–up	2

been due to failure in providing a criterion for the beginning and end of a behavior phase, and that the errors were caused, not by a subject's incapacity to make moderately accurate time signals, but because he did not have any means of knowing when a behavior phase started or ended. In any science, it is the business of a research worker to see that the criteria for the limits of behavior phases are determined before time records are taken *by any means whatever.*

Table VII gives a sample of one of the distributions of time intervals used. All of the distributions were very similar to this one. It will be observed that all of the phases were

over the .7 second critical minimum for accurate recording, illustrated in Fig. 3. The order of the intervals, with respect to length, was pure random, dependent on the whim of the mover. Tables VIII, IX and X give samples of actual errors in the order of their occurrence. So closely did the

TABLE VIII

TIME JUDGMENTS FOR ACTUAL HUMAN BEHAVIOR

Errors in milliseconds for subject FO when he is kept in ignorance of the size of his errors. Absolute error for each group of 10 judgments. Stimulus intervals between .9 and 1.5 seconds.

No.	*e*	No.	*e*	No.	*e*	No.	*e*	No.	*e*
1	15	21	−23	41	11	61	−12	81	−5
2	0	22	30	42	−50	62	−27	82	−9
3	−54	23	16	43	20	63	−32	83	24
4	20	24	11	44	−10	64	−19	84	−17
5	−24	25	−19	45	−78	65	47	85	27
6	−4	26	−8	46	1	66	−55	86	−11
7	−4	27	13	47	−52	67	−6	87	30
8	−36	28	−17	48	−10	68	−152	88	−37
9	−314	29	52	49	−46	69	−2	89	−67
10	121	30	34	50	−29	70	33	90	2
E	−4	E	12	E	−20	E	−16	E	−7
11	239	31	−30	51	−22	71	15	91	20
12	75	32	5	52	−61	72	11	92	10
13	20	33	21	53	−41	73	57	93	69
14	−68	34	5	54	−25	74	−40	94	10
15	11	35	−42	55	−28	75	−11	95	−27
16	−3	36	3	56	34	76	−50	96	42
17	20	37	−48	57	18	77	−27	97	20
18	25	38	−83	58	47	78	31	98	99
19	−43	39	152	59	1	79	9	99	22
20	−54	40	−52	60	−7	80	17	100	−2
E	16	E	−14	E	−16	E	10	E	20

Probable Error for entire table, 24; Absolute Error, +2.5.

conditions of this experiment match those in experiments on mechanical movement that the probable error is not reliably different. FO's lower probable error is not necessarily dependent on the specific conditions; he had had a far greater amount of training by this time.

The following conclusion seems justified: It is possible to keep the absolute errors of well trained subjects, based on groups of 10 responses, to 20 milliseconds or less of absolute error, less than one third the duration of a single frame of

motion picture projection at standard speed. Subjects FO and DU were well trained by this time. Subject SE was not a well trained subject and shows rather definite symptoms of a curve of learning in Table X. SE had been given only the customary 100 practice responses.

TABLE IX

TIME JUDGMENTS FOR ACTUAL HUMAN BEHAVIOR

Errors in milliseconds for subject DU when he is kept in ignorance of the size of his errors. Absolute error for each group of 10 judgments. Stimulus interval between .9 and 1.5 seconds.

No.	e	No.	e	No.	e	No.	e	No.	e	No.	e
1	−17	21	5	41	6	61	3	81	54	101	11
2	18	22	−45	42	45	62	26	82	23	102	8
3	−35	23	5	43	−2	63	−66	83	−5	103	−12
4	120	24	−14	44	−60	64	45	84	−40	104	2
5	44	25	−10	45	69	65	−6	85	−35	105	32
6	−77	26	15	46	−140	66	17	86	−40	106	31
7	−35	27	−30	47	135	67	23	87	50	107	17
8	79	28	27	48	16	68	−112	88	27	108	6
9	3	29	−38	49	15	69	52	89	−98	109	−4
10	37	30	23	50	−136	70	10	90	2	110	17
E	11	E	−3	E	11	E	14	E	−2	E	10
11	50	31	5	51	−16	71	120	91	32	111	−49
12	−34	32	1	52	62	72	60	92	19	112	−21
13	16	33	17	53	1	73	119	93	13	113	50
14	−80	34	56	54	13	74	−69	94	−18	114	35
15	−2	35	−147	55	9	75	16	95	2	115	−18
16	66	36	61	56	2	76	−69	96	2	116	−7
17	−43	37	44	57	−6	77	22	97	−44	117	22
18	50	38	−85	58	−28	78	−24	98	37	118	−50
19	50	39	6	59	26	79	−34	99	−12	119	−24
20	−89	40	18	60	42	80	−1	100	27	120	76
E	7	E	12	E	6	E	8	E	8	E	6

Probable Error for entire table, 26; Absolute Error, +6.

VIII. GENERAL CONCLUSIONS

The optimum conditions for accuracy in giving time signals for the beginning and end of behavior phases include the following:

1. The illumination must exceed the critical minimum for fine line discrimination (above 10 foot-candles).

2. The moving object must show visual contrast to the background by an amount definitely above the critical discrimination limen (30 : 1, white to black).

3. The speed of the moving object on the retina should approach one minute of the visual angle for 10 milliseconds of time.

4. The contours of the moving object must be as sharp as possible.

5. There must be an instantaneous criterion provided for the beginning and end of a behavior phase.

TABLE X

TIME JUDGMENTS FOR ACTUAL HUMAN BEHAVIOR

Errors in milliseconds for subject SE when he is kept in ignorance of the size of his errors. Absolute error for each group of 10 judgments. Stimulus intervals between .9 and 1.5 seconds.

No.	*e*	No.	*e*	No.	*e*	No.	*e*
1	58	21	−68	41	−22	61	−19
2	−68	22	53	42	−133	62	57
3	−86	23	24	43	−23	63	14
4	−102	24	124	44	−13	64	−30
5	−50	25	18	45	65	65	−34
6	−170	26	−39	46	−62	66	−2
7	−79	27	−19	47	80	67	8
8	−20	28	17	48	−47	68	−16
9	64	29	−27	49	−17	69	32
10	−71	30	38	50	−28	70	10
E	−70	E	18	E	−23	E	3
11	−56	31	11	51	−60		
12	−42	32	−43	52	−55		
13	−44	33	−20	53	34		
14	−11	34	−26	54	50		
15	−147	35	−45	55	9		
16	−67	36	−39	56	41		
17	−21	37	26	57	18		
18	−90	38	22	58	−32		
19	−155	39	−34	59	58		
20	−30	40	58	60	24		
E	−50	E	−2	E	21		

Probable Error for the entire table, 34.5; Absolute Error, −26.5.

6. The moving object must be continuously in view one or more seconds before the first response is made.

7. The total duration of the behavior phase must be in excess of .7 second.

8. Observers of behavior must be so selected, by test, that those with favorable capacities are known (from data shown, not difficult among college students).

9. In variable movement no portion of a phase must fall below the critical margin for speed.

10. Time observers must be given practice until their capacities have reached constancy, approximately.

11. The mode of responding to beginning and end of behavior phases must be made as nearly identical as possible, or the differential error of different types of responses must be known.

(Manuscript received November 14, 1936)

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ADAPTATION, AFTER-EFFECT, AND CONTRAST IN THE PERCEPTION OF TILTED LINES. II. SIMULTANEOUS CONTRAST AND THE AREAL RESTRICTION OF THE AFTER-EFFECT

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Introduction

The preceding paper has reported the fact that visual lines undergo what may be termed adaptation followed by a negative after-effect with respect to their direction or orientation in the frontal visual field.[1] A line possessing the characteristic of *tilt* appears to become less tilted during the course of a continued inspection, and thereafter a line objectively vertical or horizontal appears to be tilted in the opposite direction. In many respects the adaptation behaves like sensory adaptation of the sort exhibited by color and temperature. Furthermore the preliminary observations suggested that the negative after-effect behaved like a negative after-image in being confined to the stimulated portion of the retina. It did not seem to produce any tilt of the visual field as a whole nor any disturbance in the subject's posture but was instead probably restricted largely to vertical and horizontal lines of approximately the same locus and visual angle as the tilted stimulus-line.

In order to demonstrate that linear direction is functionally akin to a sensory process like that of color, two further investigations need to be made. It might be possible, first, to demonstrate the occurrence of simultaneous contrast in this type of perception. And second it should be shown conclusively that the process of adaptation is a localized process within the visual field. For if the vertical and

[1] James J. Gibson and Minnie Radner, Adaptation, after-effect and contrast in the perception of tilted lines. I. Quantitative studies, this JOURNAL, 1937, 20, 453–467.

horizontal axes can be temporarily shifted by a process of adaptation in one visual region, say of 10° visual angle, and at the same time remain unaffected in outlying regions, then it would scarcely be possible to argue that the effect was merely an 'illusion of judgment' or a type of 'ambiguous perception.' Furthermore, such a demonstration would distinguish the effect from those experiments having to do with the supposed cues which make possible the 'perception' of the visual vertical. The visual field as a whole possesses as one of its primary characteristics an implicit vertical-horizontal frame of reference, and the determinants of this total frame of reference have been sought in a number of theories. Kinæsthetic and equilibratory mechanisms undoubtedly play a role in this determination, along with visual factors. But the study of local and seemingly autonomous modifications of the subjective axes would be an independent problem demanding investigation in its own right.

I. Simultaneous Contrast in the Perception of Linear Tilt

It was argued in the preceding report that a visual line or segment could be said to have two fundamental qualities, shape (curved or straight) and direction (vertical, horizontal, or tilted). Both of these qualities manifest successive contrast, and curvature shows simultaneous contrast.[2] If the analogy is complete, tilt should also show simultaneous contrast.

Many everyday observations support the hypothesis of simultaneous tilt-contrast. On a street which runs sharply up hill it is often noticeable that objectively horizontal lines—the foundation lines of the houses for example—appear to tilt against the incline. Against the perceptibly tilted wall in a room of an old house which has settled to the right, a plumb-line may appear not vertical but slightly tipped to the left. It is not surprising therefore that if one draws a 30 cm line in the middle of a sheet of cardboard squarely in line with the

[2] J. J. Gibson, Adaptation, after-effect, and contrast in the perception of curved lines, this Journal, 1933, 16, 15 f.

upright edges, and then overlays it with 40 cm parallel lines at an inclination of about 10°, the line no longer looks vertical (or horizontal). A slight opposite tilt has been induced, and the line does not now appear normally oriented within its frame. It should be remarked that the induced tilt is not effective for the entire visual field or even for the edges of the cardboard but instead is confined largely to the line.

For the purpose of measuring this induced tilt, the apparatus already described in the first paper was used with only one addition.[3] A tilted grille or grating of parallel black lines was introduced into the circular field of the subject 5 cm in front of the white disk which bore the adjustable black line on the white field. The grille was made of strips of black paper ½ cm wide and 4 cm apart fastened to a circular frame outside the field of view. The subject could be required to set the adjustable line to vertical before and after the tilted grille had been superposed on it, or better, could make adjustments with the grille tilted alternately to the right and to the left, the factor of constant error being thereby avoided.

Since we wished to know the amount of tilt-contrast produced by different inclinations of the grille, three degrees of inclination were studied, 10°, 20°, and 45° to right or left of the vertical. It was also desirable to know whether the grille would induce an effect when the line was horizontal as well as when the line was vertical so measurements of both were made. For each subject, 12 adjustments of the vertical and 12 more of the horizontal were made at each inclination, of which 6 were with the grille to the right and 6 with the grille to the left. Four subjects went through the experiment. Individual adjustments of the line were made from a starting point 5° off the reference-axis, alternately to right and left.

The results are given in Table I. The averages at the bottom of the table show in the first place that with the grille tilted at 10° there is a definite contrast-effect on a vertical line. In other words, if the line seen through the grille is to appear vertical to the subject it must be set in a position tilted about

[3] The data of this experiment were obtained by Miss Doris Robinson in collaboration with the writer.

TABLE I

AMOUNT OF CONTRAST IN DEGREES AT THREE DIFFERENT INCLINATIONS OF THE INDUCING GRILLE ON THE VERTICAL AND ON THE HORIZONTAL AXIS

Subj.	10° R. or L.		20° R. or L.		45° R. or L.	
	Vert.	Horiz.	Vert.	Horiz.	Vert.	Horiz.
Ro	1.36	.36	.25	−.06	.44	.36
Re	2.18	1.27	1.19	1.00	.47	.31
Be	2.12	1.15	.97	.11	.08	.05
Br	2.35	1.50	.53	1.03	−.68	−.53
Average	2.01	1.07	.74	.52	.08	.05

two degrees in the same direction as the inducing lines; a vertical line would appear tilted two degrees in the opposite direction. If however the grille is set at 20° the contrast is very much reduced and the consistency of the paired scale-readings is lowered. At 45° the effect is small and variable. This relation between simultaneous contrast and the degree of the inducing tilt is suggestive of that found for successive contrast.

The effect on the horizontal, as shown in the table, is also suggestive of earlier results. When conditions are adequate for contrast applying to the vertical, a similar but usually smaller effect shows itself for the horizontal. Here is further evidence that the two axes function as mutually related spatial standards with a limited degree of independence.

It was discovered after this experiment had been performed that similar results, obtained under similar conditions but in a different connection, have been reported by Krantz.[4] He has described the phenomenon and published a table and graph of his results which differ from ours only in showing greater amounts of the effect, and a greater variability. His subjects were few, and were mostly children.

One may conclude that the effect of a visual field containing tilted lines on an upright line in that field is one of contrast.

[4] F. Krantz, Experimentell-strukturpsychologische Untersuchungen über die Abhängigkeit der Wahrnehmungswelt vom Persönlichkeitstypus, *Zsch. f. Psychol.*, 1930, Ergbd. 16, 171 ff. Krantz refers to an earlier experiment on this same phenomenon by Hofmann and Bielschowsky but the writer cannot find it from the reference given.

It should be noted that *no subject reported the grille as becoming vertical*, although one did state that he felt as if the lines were his whole world—as if he were in space with them. Presumably these lines underwent adaptation but complete adaptation did not occur. This was true even when the objectively vertical line was excluded from the field.

The contrast phenomenon described is related to certain phenomena described by Koffka [5] in which the principal lines of the visual field, although tilted from the gravitational vertical, may apparently *become* phenomenally vertical and horizontal under certain circumstances, that is to say, may determine the spatial framework. Obviously, however, our phenomenon is not the same thing since the grille did not dictate the subject's spatial framework. Reference will be made to this problem later.

II. The Restriction of the After-Effect to a Particular Locus within the Visual Field

It will be remembered that the adaptation was first investigated with a tilted line drawn on a visible rectangle of cardboard, the latter seen in a stable upright visual environment. A vertical line in the same region of the field as had been occupied by the adaptation-line appeared tipped, but other vertical and horizontal lines in the field did not seem to be affected and certainly the visual world as a whole did not appear to be tilted. The subject's postural equilibrium was not modified so far as could be discovered. In subsequent experiments all vision of the upright environment surrounding the stimulus-line was excluded, but there was no noticeable change in the phenomenon. When, having inspected a tilted line under these circumstances, the subject took his head away from the cone restricting his vision and looked about the room there was no apparent tendency for the room as a whole to appear tilted.

These observations pointed to a relatively localized occurrence of the adaptation and the negative after-effect rather than a generalized one applying to the entire visual

[5] K. Koffka, *Principles of Gestalt Psychology*, Harcourt Brace, 1935, Ch. 6.

environment. A systematic set of observations was required on this point, however, and so the following experiment was performed. A 15 cm tilted line was drawn inside a 25 cm square of cardboard having its edges outlined in black, and the latter was pivoted to a 56 cm square cardboard screen. The line was thus 'framed' by both the adjustable 25 cm square and the 56 cm square. This was the adaptation-figure. A second device of this sort was made, different only in that the 15 cm line was drawn vertical. This was the test-figure (Fig. 1, I). The two figures were exposed successively upon

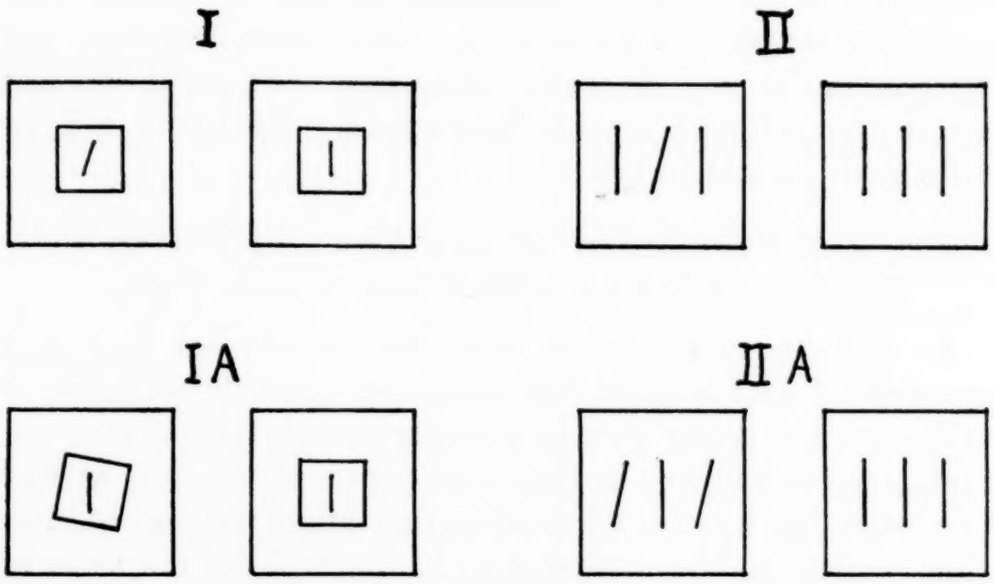

FIG. 1. The adaptation and test-figures used in this experiment. (In each case the figure on the left was looked at for four minutes and then the appearance of the figure on the right was reported.)

a large rectangular screen set in the middle of one wall of a room at a fixation distance of 46 cm. The subjects, after first assuring themselves that the test-line appeared vertical and in alignment with its frame, fixated the midpoint of the tilted line for 4 min, and then closed their eyes for 1 sec while the adaptation-figure was removed and the test-figure beneath it disclosed. They opened their eyes, fixated the objectively vertical line, and reported the appearance of the field. In general, the line showed a negative tilt and the square frame around it appeared normally oriented. The square could be rotated slightly until the line seemed vertical (1°–2°) but then

the square appeared tilted; if the square was returned to an upright position then the line looked tilted. In short the relationship between the line and the frame had been altered. On a few occasions an observer stated that the square itself looked very slightly tilted in a negative direction, but not as much as the line; the square and the line could still not be put into alignment with one another. No subject, however, reported that the larger square (56 cm) was even slightly tilted, nor were the more peripheral lines affected in any way.

Even more striking results were obtained when the adaptation-figure was arranged so that the square was tilted but the line was vertical (Fig. 1, I*A*). The subjects fixated the midpoint of the line, as before. During adaptation, some degree of simultaneous contrast on the line was usually reported, that is, the line seemed tilted in the opposite direction from the square, and this contrasting tilt was sometimes reported to increase in amount during the 4 minute period along with a decrease in the apparent tilt of the square. When the subject subsequently fixated the midpoint of the test-figure, the square frame appeared very definitely tilted in the opposite direction. So long as fixation was maintained the square continued to look tilted, but if the eyes left the mid point of the line and moved along the sides of the square it appeared normally oriented. The effect was extraordinary—a square visibly tilted at one instant but upright if one looked at the sides to make sure. The orientation of the fixated line within the square in these circumstances was reported to be approximately vertical by some subjects and by others to be tilted in a direction opposite to the negative tilt of the square (clockwise in the diagram of Fig. 1). The latter effect could be accounted for in more than one way; possibly it is simultaneous contrast induced by the apparent tilt of the square.[6]

The two experiments described were now varied by drawing the fixation-line between two lines of equal length instead of within a square (Fig. 1, II and II*A*). The same procedure was used. In II the negative after-effect appeared on the

[6] It may be significant that the analogous simultaneous contrast obtained when one employs a colored negative after-image is very vivid. The induced complementary color is usually as saturated as the color of the negative after-image itself.

middle line when it was fixated, but either not at all or to a very slight degree on the outer lines. If fixation was moved from one line to another, the fixated line showed tilt and the others appeared approximately vertical. In II*A* the negative after-effect appeared on the two outer lines but the middle line seemed if anything tilted in the opposite direction. The results are, in short, quite analogous to the previous ones.

The results of these experiments were based on the reports of four subjects each of whom observed in a part of the series, and two more subjects who went through the entire series of observations in a balanced order. Both horizontal and vertical after-effects were tested with each variation, although for simplicity the results have been described in terms of the vertical effect only. The results for the horizontal were if anything, even more clear-cut than those for the vertical. Observations were made with a low illumination in order to reduce after-images of the fixated line.

It may safely be concluded that irradiation or spread of the negative after-effect in the visual field, while probably present to some degree, is at least limited in its extent. The normal uprightness of objects in the visual field as a whole tends strongly to be maintained even in the face of a distortion of this orientation produced in one region of the field. Adaptation and after-effect tended to occur in our experiments within a local and partial frame of reference. Strictly speaking, one may not think of this localization of the after-effect as *areal*, like that of after-images, since a line has no area. The observation made at the beginning of the first report that a negative after-effect seen on a line at one fixation distance diminishes or even disappears when the head is moved to a greater or lesser fixation distance supports this proposition. It would probably be more accurate to say that the foregoing experiments indicate that the vertical-horizontal frame of reference, which normally permeates the whole visual field, may be locally modified in the particular way which the adaptation-figure dictates, but not in any other way. The partial independence from one another of the vertical and

the horizontal axes themselves which was described in Parts III and IV of the first report is in line with this interpretation.

The argument might be advanced that the adaptation-figures of I and II in Fig. 1 were of such a sort as to *prevent* the tilt-adaptation from 'spreading' over the whole visual field. The upright square enclosing the 15 cm tilted line might in some way keep the effect localized in a way which would not occur were the square removed from the field. This possibility did not seem very likely in view of the reported normal appearance of the room after earlier experiments with the line in a restricted circular field. But in order to meet it, a single 15 cm tilted line was substituted for the adaptation figures of I and II. The test-figures were unchanged. The after-effect still showed itself clearly on the fixated line but minimally or not at all on outlying parts of the figure.

Transfer from One Eye to the Other.—The negative after-effects of perceived movement and of linear curvature appear in the corresponding area of the field of the other eye when only one eye has been stimulated; the negative after-image of color or brightness, on the other hand, is specific to the stimulated eye.[7] A number of tests made with three subjects, employing the tilt-apparatus described in earlier experiments and an eye-patch which could be shifted from one eye to the other, indicated that the tilt-effect does transfer to the corresponding region of the unstimulated eye. As compared with a control series which measured the after-effect in the stimulated eye alone, the transferred effect amounted to 57 percent, 83 percent, and 65 percent for the three subjects tested. If one supposes that different types of perception may depend on different neurological levels of the visual system, then orientation is a higher (less peripheral) process than color and brightness.

It may be mentioned that the after-effects obtained in the control series where only one eye was used did not differ from those of previous experiments where both eyes were used.

[7] J. J. Gibson, Adaptation, after-effect and contrast in the perception of curved lines, this JOURNAL, 1933, 16, 25 ff.

Discussion

M. D. Vernon [8] reported in 1934 an experiment which took its origin from the fact that a bent line behaves like a curved line in that, after fixating the bend, a substituted straight line looks bent the other way.[9] She believed that the explanation was to be found in a tendency for each of the arms of the bent line independently to straighten up and become vertical rather than a tendency for the whole line to become rectilinear. The simplest test of this hypothesis would have been to see if the adaptation and after-effect with respect to rectilinearity still occurred when the bent line (and the subsequently seen straight line) were inclined 45° from the vertical. The tendency of the arms of the bent line toward vertical or horizontal is here in opposition to the tendency of the whole line to become rectilinear. The writer has frequently repeated this experiment; the after-effect of either a bent or curved line shows up independently of the orientation of the line. The only requirement is that the adaptation line and the test line shall be in the same orientation. The after-effect of a linear curve or bend is therefore separable from the after-effect of a linear tilt.

Vernon, however, went on to verify the tendency for an inclined line to become vertical. Her subjects looked with one eye for 10 minutes at a line (or a set of parallel lines) tilted 10° from the vertical in a restricted circular field. Before and immediately after this period the line-figure was adjusted to the vertical position. The differences between these two adjustments were consistently in the expected direction; six subjects yielded varying amounts of after-effect which averaged about 2.5°. Our results are perfectly in agreement with hers. She obtained a somewhat higher average amount of effect, but differences in procedure and the fact of individual differences among subjects are probably adequate to account for any discrepancy.

[8] M. D. Vernon, The perception of inclined lines, *Brit. J. Psychol.*, 1934, **25**, 186–196.

[9] J. J. Gibson, *op. cit.*, 18 ff. The phenomenon has also been noticed by F. H. Verhoeff, A theory of binocular perspective, *Am. J. Physiol. Opt.*, 1925, **6**, 436.

Vernon did not explore any of the aspects of the phenomenon with which the present report and the previous one have been concerned. She apparently assumed that the adaptation and after-effect could be obtained only in a restricted circular field where the line itself was the only visible direction. Her subjects looked through a circular hole in a screen, using one eye only. She explained the phenomenon as a shift of the general spatial level or framework,[10] and this explanation requires consideration. Koffka has pointed out that phenomenal space serves not only as a ground against which objects are seen but as a frame of reference for their orientation and location. Lines and objects in the frontal plane are seen as upright or tilted by virtue of the horizontal-vertical framework. According to this theory, however, not only does the framework provide a reference base for the position of visual lines and objects but it is itself determined by the main lines of the visual field—the principal visible directions. The horizon-line would be such a principal direction. These main lines of the field, even if by chance or by experiment not gravitationally vertical and horizontal, will become so phenomenally because they determine the framework. Experimental evidence for this theory comes from Wertheimer.[11] He performed an experiment in which the subject looked through a tube into a mirror-surface so tilted as to present to his vision an image of the room inclined at about 45° from the objective vertical. After an interval the room was reported to become normal in appearance; it was upright because the main lines had become the subject's framework.

Vernon adopts this theory as an explanation of the tilt-effect. She supposes that in her experiment the tilted line provided the subject's framework. She adds that "this spatial framework tended to appear as vertical as possible, *i.e.* to move towards the vertical position." [12] As stated, this explanation is self-contradictory for if the line was vertical

[10] K. Koffka, Perception, an introduction to Gestalt theory, *Psychol. Bull.*, 1922, **19**, 531, and *Principles of Gestalt Psychology*, Ch. 6.

[11] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 1912, **61**, 257–262.

[12] M. D. Vernon, *op. cit.*, 190.

(*i.e.* provided the framework) it could not tend "to appear as vertical as possible." As a matter of fact, the subjects in our experiments did not report that the line ever looked unmistakably vertical and remained so. They did report that it fluctuated and that it became less tilted. It is possible that this criticism is mainly a verbal one. Just what Vernon meant to imply is not clear.

But in any event, the theory of a labile spatial framework shifting in accordance with the dominant visible directions is not really applicable to the phenomenon which we have investigated. In the first place, the adaptation is partial rather than complete. In the second place, the effect is nonexistent at 45°, and hence is different from the phenomenon which Wertheimer observed. Both of these facts were established in the first paper. Finally, as we have just seen, the adaptation with after-effect can occur in a localized part of an otherwise stable visual field. The spatial framework *as a whole* is not affected.

The evidence obtained in these experiments seems to point toward a somewhat different interpretation. The effect behaves like a partial and local adaptation process akin in many respects to sensory adaptation. Color and brightness, linear shape, movement, skin temperature, and probably other such dimensions of human perception are parallel in this respect to linear orientation. A theory of the phenomenon of tilt-adaptation and after-effect should, if possible, be in such terms as will include these analogous phenomena. An attempt at a general theory of this sort requires a separate paper. Only an indication will be given of the concepts which could be employed and their application to linear orientation.

The vertical and horizontal axes, as we have seen, are implicit in visual perception. They can be produced without visual cues, and presumably their *ultimate* determinants are postural. In the frontal visual plane lines have the quality of direction with respect to these axes, and only with respect to them can directions be discriminated. A tilted line is one which deviates from, say, the vertical in one of two regular ways, to the right or to the left. A vertical line on the other

hand is simply one which is not tilted in either of these ways. 'Vertical' and 'tilted' are definable only in terms of one another. They may be said to constitute a single sensory continuum. The term 'sensory' is applied not in its older systematic meaning but only to indicate a simple character of perception. A number of reasons could be given for terming both linear direction and linear shape sensory but their relative simplicity is sufficient reason for the present discussion. Correlative with the vertical-tilted continuum there is of course a quantifiable stimulus continuum, based on the gravitational vertical and horizontal and measurable in angular degrees.

Now this sensory continuum, in common with some others already mentioned and also with a large number of more complex dimensions of experience, consists of a central norm or standard between opposite qualities. The central quality is neutral with respect to the others and is unique. The two opposite qualities become increasingly intensified as they deviate from the norm. The opposition of the two qualities consists in the fact that they cannot coexist in the same object at the same time, and if one attempts to modify the stimulus in both ways at the same time, the neutral quality results.[13] The opposite qualities might be designated as plus and minus, the norm as zero. The series is symmetrical, and it may or may not have specific limits or ends. In the case of linear orientation, there are two such 'opposition series,' related to one another, one for vertical and one for horizontal.

In these general terms tilt-adaptation would be a process in which the normal correspondence between the stimulus-continuum and the sensory series is altered. A prolonged inspection of a tilted line results in a decrease in the apparent tilt. This means that a retinal tilted line now corresponds to a phenomenal less-tilted line. And since, as we have seen, the vertical-tilted series is 'all of a piece' it follows that a retinal vertical line corresponds to a phenomenal oppositely-tilted line. This last is of course the negative after-effect proper. Likewise, a line to be seen as vertical must be tilted

[13] For a discussion of various types of opposition in relation to series or scales, see C. K. Ogden, *Opposition*, Psyche Miniatures, Gen. Series, 1932, No. 41.

somewhat with the same quality as the adaptation-line.[14] The relationships are represented in Fig. 2.

There is a corollary of this theory. The question of whether the tilted line has tended to become vertical or whether the subjective vertical has tended to become the perceived line is not only indeterminate but is a false problem. One cannot simply say that during a prolonged perception, the tilt quality of a line is shifted toward the norm of its series. That is true, but it would be just as true to say that instead the norm is shifted toward the perceived quality. The norm and the series are mutually dependent; they are relative to one another and neither the one nor the other is primary. The two statements, therefore, come to the same thing. A better statement than either is that during a prolonged per-

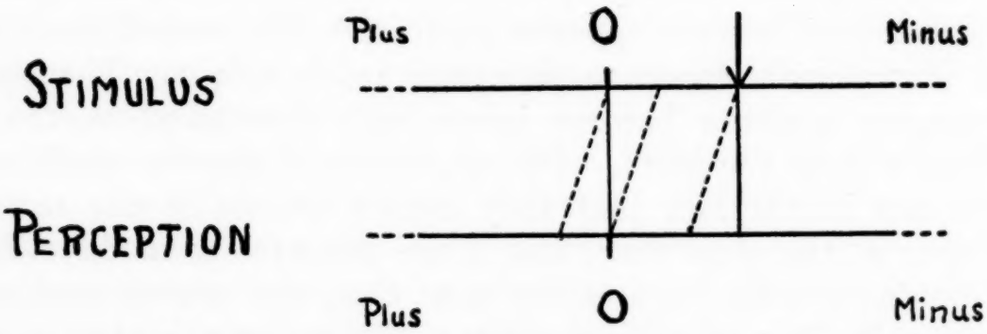

FIG. 2. Diagram of the relations between the stimulus-continuum and the sensory series before and after a period of tilt-adaptation. (The arrow indicates a prolonged stimulus-presentation. The solid lines indicate the correspondence between retinal and seen tilt at the beginning, and the dotted lines at the end, of the adaptation period.)

ception the correspondence between the sensory series and the stimulus series has been altered throughout in such a way as to reduce the discrepancy between the norm and the perception. The actual stimulus is now correlated with a subjective quality nearer its norm, and this norm is now correlated with a stimulus-value closer to that of the actual stimulus. Presumably when the stimulus-line is removed from the field the process by which these correspondences were changed is now reversed and there is a return to the usual relationship. This process of adjustment and readjustment of the retinal-sensory

[14] *Cf.* J. J. Gibson, *op. cit.*, p. 7 and Fig. 2.

correspondence, it should be remembered, is partial rather than complete. In the first place it tends to be limited to the region of the visual field occupied by the non-normal perception. And in the second place the discrepancy between the norm and the perception does not disappear but only decreases.

This tendency of a perceptual quality toward the norm of its series should not be identified with the tendency of perceived and remembered forms toward types, centers, or good configurations. Studies of perception by the method of immediate or delayed reproduction or by the method of recognition, usually with reduced stimulus-conditions, have to do with the determinants of perceptual 'errors' or memory-changes; the 'method of prolonged perception' has to do with a change of the retinal-phenomenal correspondence itself. A study of the perceptual orientation of forms by the first method has been made by the writer.[15] The similarity of the results in the two experiments should not disguise the difference in the problem investigated.

The theory has been proposed that successive contrast is a process which involves a sensory 'opposition' series. The prolonged perception of a particular quality not the norm of its series results in a gradual shift of the serial correspondence between stimulus and quality of such a sort as to reduce the abnormality of the particular quality. Simultaneous contrast, such as was observed in the first experiment reported, might be described in the same terms. We can suppose that the perception of a non-normal quality over a large area of the visual field has an immediate effect upon the quality of an enclosed region of differing stimulus value such that the quality in this region is shifted from its usual stimulus base in a direction opposite to the surrounding quality. There is a difference between these two formulations, however. The last one is in no sense explanatory of the contrast-effect as the first one is of the negative after-effect. Although the simultaneous contrast phenomenon as it was observed in this experiment probably also involves the notion of a sensory opposition series and bears an intriguing resemblance to the

[15] M. Radner and J. J. Gibson, Orientation in visual perception; the perception of tip-character in forms, *Psychol. Monog.*, 1935, 46, No. 210, 48–65.

after-effect phenomenon, it is far from being the same thing and requires some hypothesis of its own.

It should be pointed out in conclusion that this experiment has dealt with linear orientation under the special conditions of a frontal plane stimulus-line and an upright posture of the head. Linear orientation in the third dimension has been set aside for separate experimental treatment, although the writer believes that it too manifests the same general phenomena. Likewise the fact of a 'constancy' of linear direction—its strong tendency to remain phenomenally unchanged when the head and retinae are tilted and the special case illustrated by the Aubert phenomenon—has been left out of account. In our experiment an objectively vertical line was always correlated with an objectively upright retinal image. The stimulus-continuum which has been referred to should therefore be understood in every case as the stimulus-continuum-with-the-head-upright. Ultimately these experiments should be extended to include conditions of abnormal posture.

Summary

1. Linear directions in the visual frontal plane are perceived by reference to the norms of vertical and horizontal which remain stable and precise in an otherwise homogeneous visual field.

2. A perceptual tilt which lasts over a period of time results in a reduction of the apparent tilt and a subsequent negative effect on an ordinarily normal stimulus-line. The adaptation and after-effect, so-called, have been interpreted as a partial adjustment with subsequent readjustment of the retinal-sensory correspondence.

3. The vertical and horizontal norms and their two respective series of tilt-qualities may be said to constitute a single system and yet to operate in partial independence, since an after-effect on one norm transfers only in part to the other norm.

4. The effect occurs in that region of the field of vision where a tilt-perception has been maintained; the field as a whole remains unaffected.

5. Simultaneous as well as successive contrast is operative.

6. Linear direction is similar to linear curvature or shape with respect to a number of common functional characteristics.

7. The hypothesis is suggested that a number of types of perceptual experience are subject to adaptation with negative after-effect, namely those which fall into what has been called an opposition series.

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INITIATION OF THE REPRESSION SEQUENCE BY EXPERIENCED FAILURE *

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According to the repression hypothesis as restated in terms of reaction psychology (Sears, **2**), the fact of repression results from a conflict between mutually incompatible anticipatory goal responses when the anticipatory response to punishment or failure is dominant, *i.e.*, overcomes the anticipatory response to reward or success. Repression itself is the *blockage of the acts*, either pure stimulus or contributory, which lead to the drive-satisfying goal. It may manifest itself in degrees varying from a simple decrease in efficiency of these acts to a complete blockage of them. In this latter case the disability is described as an amnesia, particularly if the verbal accompaniments of the acts are blocked as completely as are the acts themselves.

The present experiment was devised to provide two indirect tests of the development of these antagonistic anticipatory responses. The first criterion of their presence in an appropriate experimental situation is a decrement in the efficiency of the pure-stimulus or contributory act sequence. The second criterion is a measured interference with a second activity, which is temporally contiguous to the sequence, when stimulational conditions are such that the antagonistic responses should, if potentially existent, be excited.

METHOD

A card-sorting task measured as a speed test was used as a competitive activity. To provide reinforcement or frustration of the drive for success-in-competition half the subjects were allowed to 'succeed' and the other half forced to 'fail.' The learning of series of nonsense syllables before and after the card-sorting was used as a second

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activity on which the effect of interference from the hypothetical antagonistic anticipatory response could be measured.

Subjects.—A group of 20 male freshmen working throughout the semester as members of an NYA project was used. The morale of the group was kept exceptionally high by the usual techniques for group motivation: delegation of minor responsibilities, encouragement, pep talks on the group's contributions to science, development of intra-group competition, etc. The relative immaturity of most of the subjects made this task simple.

Procedure.—During the course of an experiment on levels of aspiration (Snedden, 3) the subjects, working individually, practiced the card-sorting task 10 times on each of 3 days. The task involved the sorting of a single deck of playing cards into the 4 suits. The suit symbols were painted on a table and each card was to be placed under the symbol representing the suit of the preceding card played. The first card would always be placed arbitrarily under the club symbol and the second would be placed under whatever symbol was indicated by the first card; the third card was placed as indicated by the suit of the second card, etc. The score for the task was the number of seconds, as measured by a tenth second stopwatch, required to sort a deck of 52 cards. Subjects were told their scores after each trial and were asked to set a level of aspiration for the succeeding trial.

The day before beginning the present experiment each subject memorized by the method of anticipation 3 lists of 10 nonsense syllables of uniform average meaningfulness (7 percent) taken from Hull's list (1). The syllables were presented on cards, manually, in time with a metronome, each syllable being exposed 2.3 seconds. The subjects were divided into 2 groups of 10 each by being paired in terms of (*a*) average time for card-sorting on the last 20 of the 30 preliminary trials, and (*b*) number of repetitions required for memorizing the last 2 lists of nonsense syllables to a criterion of 2 consecutive correct repetitions.

Each subject served in the present experiment on 3 occasions not less than 24 hours nor more than 48 hours apart. On each occasion a list of nonsense syllables comparable to the ones used for preliminary practice was learned first. Then followed 15 trials at the card-sorting. The session ended with the learning of another list of nonsense syllables. The same lists of syllables were used for the various occasions with all subjects. Treatment of the 2 groups was the same except that one was made to 'succeed' on the card-sorting and the other to 'fail.'

Success and Failure.—A rational analysis of the process of failing at a semi-competitive task seemed to indicate that failure is a result of *comparison of performances* or *ability* for performance. It was conceived that 4 types of comparison are chiefly responsible for developing the feelings of failure or success:

1. Comparison of present performance with immediate past performance on the same task.
2. Comparison of present performance with average of own past performances on the same or similar tasks.
3. Comparison of present performance with the level of aspiration.

4. Comparison of own performances, past and present, with the performances of other members of the group; this of course presumes a feeling of potential equality among the members of the group.

In order to produce feelings of failure at the card-sorting with one group and feelings of success with the other, the experimenter reported scores falsely, in one case keeping the reported scores well below and in the other well above the level of aspiration as set for each trial by the subject. Subjects were told false averages for the rest of the group and an attempt was made, so far as was practicable without arousing suspicion, to report each succeeding trial as slower with the members of the Failure group and faster with the Success group. Since all subjects knew their scores on the preliminary trials it was not difficult to make them succeed or fail in comparison with the averages of their past performances. The experimenter commented freely on the performances, using any device at any given opportunity for furthering the development of feelings of failure or success as the case might require.

These techniques proved more than adequate for 9 of the 10 subjects in each group. The following notes and spontaneous comments recorded during the experiment indicate typical reactions to the situation.

Success.—"I hate to say 28, but I will!" laughing self-consciously; face very red. Laughs very hard: "I'm afraid to say 31 for fear I'll blow up, but I'll try it." Laughed hard; nearly crazy with excitement; very flushed and shrill-voiced, "I can't believe it!" "I'll give the boys something to shoot at!" "My stock's going up." "Sure working under a lot of pressure." 'Whoops!' sweeping gesture across brow. "*Hot* dog! I *knew* it!" after being told he had broken all records. "We'll get it this time!" "We'll get her down there this time!" Smiled broadly and ruffled deck with gesture of self-confidence. Told this was best score in 1800 trials; much impressed, said, "That's quite an honor!" "I'll try for 29—but don't think I can make it." Excited laughter: "I'll try again!"

Failure.—"Worst I've ever done." Dropped head on desk; started to explain why he's so slow. "It's getting to be an ordeal. Don't know what's the matter." "If I did it once I can do it again but the cards are against me." 'Whew! gosh!' "Your stopwatch has a grudge against me." Dropped head on desk. "I'm getting disgusted with myself." "My eyes are rather bad." Considerable resentment; thinks it can't be done any faster. Complains of getting all mixed up; face flushed and refuses to look at E. Slammed cards on desk; rolled up sleeves. "Do I have another day of this? I'll have to get better." "I get pooped out just sitting here. Let's hurry up."

Appears sullen; voice dry and hushed. Pouting. Sighs heavily; obviously dislikes task extremely. Slumps in seat. Grimaces and sighs. Very bored; yawned ostentatiously and looked around room. "Yes, I really *am* slow." "I'll try for 58—do my best anyway." "I did better than that before; something's happened to me." "Seems like I'm just stuck at 60; can't get below it." Blew nose strongly and turned face away; crying? After last trial started to describe all his faults; voice broke several times. Looked at ceiling with peculiar smile; teeth clamped shut—"I've always been very nervous—I was sick once—." Sagged in chair, gasping and sighing. Lips trembling; breathing hard: "Not much sleep last night; been in class all day." Dropped head on hands. "Practice doesn't seem to do any good." Hands trembling badly; voice broke. Swore viciously under his breath at an error which slowed him. Sullen. Heavy sighs and swearing under breath. "I give up!"

Subjects were late for 4 of the 30 appointments in the Failure group and 1 appointment was forgotten entirely. Only 1 appointment was not kept promptly in the Success group. One S in each group reacted inadequately so far as the purposes of this experiment were concerned. The 'Success S' had been the slowest card-sorter of the entire group on the preliminary trials and knew it from conversations with the other S's. He maintained a strong feeling of inferiority throughout the present experiment in spite of the experimenter's best efforts to induce a feeling of success. Since, in addition, he did not complete the 15 trials on the first day his scores have not been included in the results on card-sorting as given below (Table 1); the Success group is therefore based on 9 S's. The 'Failure S' evidently felt some failure on the card-sorting but developed a strong compensatory reaction toward the nonsense syllable learning, using it as a second task on which he might be able to recover his prestige. The conditions of the experiment demanded that the verbal learning task have no aura of competitiveness; the learning data have therefore been presented both with and without the inclusion of his scores (Table 2). His card-sorting scores are included in the means of Table 1.

Results

1. Efficiency of Card-sorting.—In Table 1, columns 1 and 7, are given the mean card-sorting speeds for the 2 groups on each of the 3 experimental days. Each of these values is the mean of the several S's' medians of the 15 trials on each

of the 3 days. The remaining columns give the means, for the 2 groups, of the 15 trials each day taken consecutively three trials at a time. Thus column 2 represents the average speed of the Success group on the first 3 trials on each of the 3 experimental days; column 8 represents the same data for the Failure group. The sixth and twelfth columns represent the means of the last 3 trials for the corresponding groups.

Two exceptions are to be noted: the means for Day I for the Failure group do not include data for 1 S who completed only 10 trials; inclusion of these few values would not materially alter the size or direction of change of the presented

TABLE 1

MEAN NUMBER OF SECONDS REQUIRED FOR CARD-SORTING BY SUCCESS AND FAILURE GROUPS ON EACH OF 3 DAYS, TOGETHER WITH MEAN TIME OF CONSECUTIVE TRIALS TAKEN 3 AT A TIME ON EACH DAY SEPARATELY

Day	Success (N = 9)						Failure (N = 10)					
	Trials						Trials					
	M	1–3	4–6	7–9	10–12	13–15	M	1–3	4–6	7–9	10–12	13–15
	1	2	3	4	5	6	7	8	9	10	11	12
I	40.3	43.3	41.6	41.0	39.4	38.5	44.9	44.3	44.4	46.2	45.7	45.1
II	37.9	38.0	38.0	38.5	37.9	37.3	40.7	39.6	40.4	42.0	41.6	41.2
III	35.5	35.7	36.4	36.3	34.9	35.5	40.2	38.7	39.6	41.2	40.7	41.4
M	37.9	39.0	38.7	38.6	37.4	37.1	41.9	40.9	41.5	43.1	42.7	42.6

values. Another S in the Failure group became suspicious of E's timing on the third day and, although inclusion of his scores in the means for Day III would slightly increase the difference between the Success and Failure groups, with respect to both absolute size of difference and direction of change during the course of the 15 trials, it seemed safer to eliminate him from that day's means. In the data given below on learning of nonsense syllables his learning score is based on 2 days only.

In Fig. 1 are shown the directions of change in speed during the course of the 15 trials for the 2 groups on each of the 3 days. The points on the curves are taken directly from the corresponding values in Table 1. It is clear that whereas the Success group showed a continuing practice

effect during the 45 trials, the Failure group showed practice only from day to day. Efficiency of card-sorting *decreased* rather than increased from the beginning to the end of each day's trials. A rough measure of the reliability of this difference between the 2 groups may be obtained from a comparison of them in terms of the average amount of loss or gain from the first 6 trials on all 3 days to the last 9 trials on all 3 days. The Success group evidenced a mean *gain* of 3.00

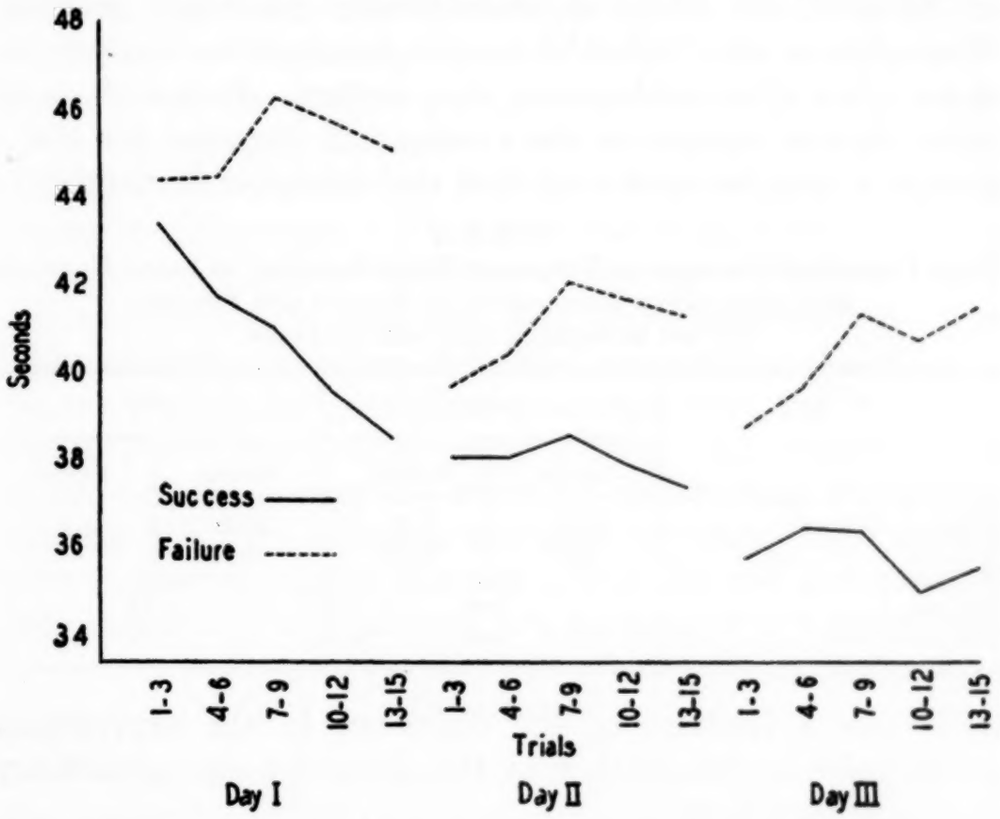

FIG. 1. Average time required for card-sorting by the Success and Failure groups on consecutive trials taken 3 at a time on the 3 experimental days.

percent (S.D., .72) while the Failure group suffered a 3.56 percent mean *loss* (S.D., 1.24). The difference is 6.56 percent (S.D., 1.44) with a critical ratio of 4.55.

2. Effect of the Failure-success on Subsidiary Learning.— It would be expected that with preliminary practice on only 3 series of nonsense syllables a continuation of the learning for 3 more days at the rate of 2 series per day would evidence further practice effect, *i.e.*, decrease in number of repetitions required for reaching the criterion of learning. In Table 2

are given as percentage values the mean decrease in number of repetitions (trials) required for learning the second series of syllables as compared to the first; these values are the means of the 3 days' measurements. It is seen that the S's who 'succeeded' at card-sorting required 21.9 percent fewer trials to learn the series given immediately after card-sorting than the series given immediately before card-sorting at the same experimental session. This decrease may be considered as in part the effect of immediately preceding practice. The subjects who 'failed,' however, required but 6.9 percent fewer trials after card-sorting than before. Inasmuch as all other factors appear to have remained constant for the 2 groups it may be concluded that the difference in number of

TABLE 2

MEAN PERCENTAGE DECREASE IN NUMBER OF TRIALS REQUIRED TO LEARN NONSENSE SYLLABLES AFTER EXPERIENCES OF SUCCESS AND FAILURE

See text for explanation of number of cases.

	10 S's		9 S's	
	Success	Failure	Success	Failure
Mean	21.9	6.9	25.9	3.5
$S.E._m$	4.58	4.08	2.47	.61
Difference	15.0		22.4	
$S.E._{diff.}$	6.13		2.54	
Critical ratio	2.44		8.82	

trials was a function of the difference in the experienced gratification or frustration of the drive for ego supremacy (competition drive).

All 10 subjects of each group are included in the means and statistical analyses of the left half of Table 2. The 2 S's mentioned above who reacted atypically to the failure and success stimulation minimize the difference between the groups seriously. The 'failure S' who compensated on the learning task showed the greatest decrease in post-card-sorting learning over pre-card-sorting learning in the failure group, his average decrease for the 3 days being 38 percent while the S showing the next greatest decrease was only 17 percent better after card-sorting than before. Likewise the 'success S' who obviously felt very inferior about his per-

formance suffered an *increase* of 14 percent in number of repetitions, being the only member of the group to show an increase. The S having the next smallest 'saving' *decreased* 14 percent—an interval of 28 percent. If these 2 S's are excluded from the means of their respective groups, the mean of the Success group is 25.9 and that of the Failure group 3.5. With this increase in the difference to 22.4 percent there is a corresponding decrease in the size of the standard error of the difference and a very much higher critical ratio (8.82).

3. Relation between Effects on Motor and Memory Tasks.— The 9 S's of the Success group were ranked (*a*) in terms of the percentage gain in speed of card-sorting on the last 9 trials on all 3 days over the first 6 trials. The range was from .8 percent to 6.6 percent. They were also ranked (*b*) in terms of the percentage gain in efficiency of nonsense syllable learning after card-sorting as compared with before card-sorting. The rank correlation between these two measures of the effect of success was + .46. A similar procedure with the data from the 10 S's of the Failure group gave a rank correlation of + .32; *i.e.*, the greater the effect on card-sorting, the greater was the effect on learning nonsense syllables. The change from the first 6 trials to the last 9 trials for this group varied from an increase of .7 percent to a decrease of 11.9 percent.

Discussion

Two facts seem evident from these data: first, that failure at a semi-competitive task produced a progressive impairment of the efficiency with which that task was performed, and second, that failure on the one task impaired the performance on another task temporally contiguous to the first and carried out in the same external stimulational setting as the first. It is suggested that both facts may be explained as the results of the operation of anticipatory responses to the failure. The formula for the development of such responses has been presented in detail elsewhere in diagrammatic form (Sears, 2); briefly, it was supposed that the drive stimuli initiating or accompanying a sequence of acts leading to a goal that removed the drive stimuli (satisfied the need)

would become, by process of association, adequate stimuli for the arousal of anticipatory goal responses. If, then, the attainment of the goal were followed by conditions which served to nullify the effect of that attainment (*e.g.*, failure), an additional set of anticipatory responses would develop. These would be anticipatory of the final responses which eventuated from the frustrating stimuli or conditions and would, by the very nature of that frustration, be antagonistic to the original anticipatory goal responses. It was further supposed that the conflict between these two sets of anticipatory goal responses would produce a disorganization of the sequence of pure stimulus or contributory acts associated with them (**2**, 251–253). This disorganization would presumably be evidenced by a decrease in the speed with which the acts were performed. The acts involved in sorting cards in the present experiment may be considered as contributory acts.

The progressive decrement in speed of sorting toward the end of the 15 trials indicates the formation of the anticipatory responses to failure during any one day's trials. Clearly they are not reactuated with their full strength at the beginning of each day subsequent to the first, since the first 3 trials of the second and third days (Failure group) are faster than the trials of the preceding day. In other words the carry-over from day to day of these newly developed anticipatory responses is less than the effects of practice. The fact that the difference between the 2 groups on the first 3 trials becomes greater each day may be significant, but the omission of 1 S on the first and third days in the Failure group precludes a direct statistical comparison.

Although the significance of the external stimuli associated with the drive stimulus was not emphasized in the writer's earlier presentation of the hypothesis, it is clear that these would serve, as well, for stimulation of the incompatible anticipatory responses. If they were presented at a time when the anticipatory responses to failure were potentially dominant over the anticipatory responses to success, the former would be initiated. Since the nonsense syllable

learning in the present experiment took place at the same desk with the same experimenter and with the deck of cards lying immediately in front of the subject, it may be conceived that the external stimuli were present during that learning process and that the anticipatory responses to failure were actuated in some degree. With instructions to 'learn the list of syllables as rapidly as possible,' the drive to achieve a goal, *i.e.*, to follow instructions in a conative activity, would presumably be initiated and this would be in conflict with the anticipatory goal responses to failure developed during the card-sorting task which was undertaken in accompaniment with approximately the same stimulation. Result of this conflict would be to decrease the efficiency of the learning process, a fact demonstrated by the obtained data.

One further point, not directly relevant to the problem of repression, may be considered. Sullivan (4) has shown that failure artificially induced by false comparisons with others decreases the rapidity with which learning of nonsense syllables occurs. In her experiment the failure was induced with reference to the task of learning itself. Similar results were obtained in the present experiment with the motor task of card-sorting. The present data on nonsense syllable learning indicate further that, at least so long as stimulating conditions remain fairly constant, there is a transfer of the effects of success and failure from the task at which these feelings are incurred to another task temporally contiguous to it.

Conclusions

1. Failure, in contrast to success, produced a progressive decrement in the efficiency with which a competitive motor task was performed.

2. Failure, in contrast to success, affected adversely the efficiency of a temporally contiguous verbal learning task carried out under conditions providing external stimulation similar to that associated with the task at which success or failure occurred.

3. Both facts may be considered as tentative verifications of an important phase of the hypothecated repression se-

quence: viz., the development of mutually incompatible anticipatory goal responses.

4. There is a slight positive relationship between the degree of effects of failure and success on the two tasks.

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TACTUAL LOCALIZATION WITHOUT OVERT LOCALIZING MOVEMENTS AND ITS RELATION TO THE CONCEPT OF LOCAL SIGNS AS ORIENTATION TENDENCIES

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Introduction

Peterson's (4) concept of local signs as orientation tendencies acquired during the course of adaptive behavior has led to renewed interest in problems of tactual localization. Dunford (1), Renshaw (5), and Renshaw, Wherry, and Newlin (6) have recently demonstrated that tactual-kinesthetic localization of points on the skin improves with practice and their results have been regarded as evidence favoring Peterson's view.

Analysis of the tactual-kinesthetic method of localization, however, shows that the observed improvement may have more than one source and that it may have no direct bearing on the modification of so-called 'local-signature' which is required by Peterson's view.[1] Let us, in order to make this point clearer, examine the tactual-kinesthetic method of localizing.

The experimenter stimulates a point on the skin and the subject, without visual aid, attempts to touch the same spot with the point of a stylus. Exploratory movements are eliminated and the distance through which the arm moves in localizing is kept relatively constant. While being tested in

[1] Lotze (3) referred to local signs as "the feelings so difficult to describe, according to which we distinguish a contact at one place from the same contact at another" (p. 63). In other words, local signature was, for Lotze, evidenced by a tactual *experience*, not merely by an accurate localizing movement. While improvement in overt localizing movements is readily observed, there is no evidence that correlated changes in local signature, i.e., changes in knowledge of the sensory topography of the skin, result from such movements.

this way, the subject quite frequently reports that he *knows* the approximate position of the stimulated spot, but that he has difficulty in guiding the point of the stylus to it. Frequently, after he has placed the point of his stylus upon the skin in an attempt to localize, the subject will say, "That is not the place. It is closer to the hand, more to the right," etc. It is apparent, therefore, that improved localization, when the tactual-kinesthetic method is used, may reside in the subject's learning to indicate with greater facility what he already knows concerning the location of the stimulated regions. Such improvement is facilitated by the knowledge of results involved in tactual-kinesthetic localization. Since the subject may compare the experimenter's stimulation of his skin with his own stimulation of it while localizing, there is constantly possible an implicit knowledge of the magnitude and direction of errors.

The subject's knowledge of the sensory topography of his skin *may* be improved during tactual-kinesthetic localization, but there is no guarantee that such improvement occurs. It appears that crucial evidence for Peterson's concept must come from improvement in localization when overt localizing movements of a differential nature are absent. The present experiment was devised for the purpose of determining whether such an improvement in tactual localization can be demonstrated.

Ewert (**2**), in his experiment on retinal inversion, found evidence of improvement in tactual localization when overt localizing movements were absent, but the improvement was in all probability merely an adjustment to the effects of inversion. There is no evidence that it involved an increased knowledge of the sensory topography of the skin.

Method

The method used in this investigation is similar, in many respects, to that of Ewert. A diagram, as illustrated in Fig. 1, was stamped on the subject's right volar forearm at a point just below the elbow joint. The diagram was traced with Jentian Violet and retouched whenever necessary. Thus the same area could be stimulated from day to day. Stimulation was always in the approximate center of a square, and each square was stimulated once daily. The subjects did not, unless they guessed it, know that each point was stimulated once in each series of 30 stimulations. As a

matter of fact they sometimes reported as many as five stimulations in a given square. The order of stimulation varied from day to day. Each series was a random one except that successive stimulations in the same column or row were avoided. Different subjects were given the series in different orders so that there would be a check on the possibility that any trend in the accuracy of localization might be due to increasingly easy (or difficult) series. There was no evidence, however, that any series was more difficult than another so long as successive stimulations in a given column or row were avoided. The series used in the first and last tests were identical.

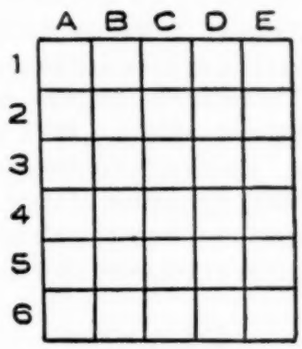

FIG. 1. Diagram stamped on the arm.

The writer, who acted as experimenter for all subjects other than himself, made an attempt to keep the stimulations of equal intensity and duration. Stimulation was applied by means of a Block æsthesiometer obtained from Stoelting. The thin wire of the stock apparatus was replaced by a ⅛ inch caliber piano wire. The pressure applied yielded a reading of 5° on the æsthesiometer. This intensity was too weak to leave any but a momentary visually apparent impression. When the subject looked at his arm, the impression had disappeared. A check upon the possibility that visual cues were involved was carried out during the last series. The illumination of the room was reduced until the subject could barely make out the diagram on his arm. Such a control did not influence accuracy of localization. The duration of each stimulation was approximately one second. The interval between stimulations approximated 20 seconds.

As far as stimulation is concerned, the method was alike for all subjects. Various methods of *localizing* were used with different subjects, however, in an attempt to find the most adequate one for the present purpose. At first the subject's arm, including the diagram stamped on it, was photographed. The arm was placed under a screen which hid it and the experimenter from view. The subject looked at the photograph while being stimulated and, as soon as the stimulus was removed, reported the square in which it had apparently been applied. In another series of experiments the subject's arm was not photographed. He looked, while being stimulated, at a diagram like the one on his arm. With both methods there was a rapid and marked improvement in localization from day to day. It appeared, however, that the experimental situation was too artificial. The observed improvement might reside merely in the subject's learning to associate the diagram on the photo, or the diagram on a sheet of paper before him, with the diagrammed area on his skin.

The method finally selected for an experiment with a number of subjects was as follows: The subject placed his arm in a designated position under a screen which hid the forearm and the experimenter. At a 'ready' signal he closed his eyes and made an attempt to visualize the diagram and forearm. As soon as the point of the æsthesiome-

ter was removed, the subject drew his arm from under the screen until his elbow touched the edge of the table. (The distance of arm movement was kept constant so that localizing movements of a differential nature would be absent.) The subject looked at the diagram on his arm and, to the best of his ability, indicated verbally (by column and row) the square in the center of which stimulation had been applied. Responses were recorded on a copy of the diagram. The error of localization was determined by measuring the distance from the center of the stimulated square to the center of the one reported by the subject. Nothing was said to the subject concerning the accuracy of his results. He was told that the results for the various subjects would be compared and that he was to do his best at all times. It was emphasized that the subject should not touch the diagram while looking at it, or have extra-experimental practice of any kind. A control of the possibility that the observed improvement was due to outside practice was carried out with some additional subjects. With these subjects the diagram was removed after each daily series. Only spots representing the outer corners remained. The diagram was replaced each day by the use of these spots.

The subjects of the main experiment were 14 male adults, 13 students in psychology and the writer. Training was given at the same time daily for 12 successive days.

Results

The results are summarized in Table 1. It will be observed that some subjects failed to improve. Others mani-

TABLE 1

Average Error of Localization in Millimeters for Each of the Daily Series of Thirty Stimulations

Days	Subjects														Average	V
	CB	RW	WM	TH	NM	PM	WS	JD	NE	GW	BR	TO	PD	GG		
1	13.3	11.4	11.0	16.7	16.5	22.0	14.2	16.3	16.7	12.5	15.7	13.9	13.9	16.1	15.0	17.9
2	11.8	10.6	9.9	14.4	11.1	18.9	12.8	12.3	15.0	12.7	12.7	15.1	11.5	12.0	12.9	17.7
3	8.9	10.4	11.7	15.7	9.7	9.9	7.9	10.9	13.9	15.3	14.4	14.4	14.5	14.4	12.3	20.6
4	12.7	8.5	12.0	18.0	11.9	21.4	9.7	13.8	11.5	15.3	10.7	13.9	11.7	15.6	13.3	24.6
5	12.3	10.2	11.1	17.8	11.6	11.6	9.1	9.9	15.5	10.6	12.7	11.7	9.6	13.0	11.9	19.0
6	9.5	10.2	12.8	14.9	9.7	9.5	12.2	12.5	15.0	9.8	12.8	11.2	12.0	15.9	12.0	17.3
7	8.9	8.3	14.1	11.9	12.9	14.6	11.1	13.7	10.7	13.3	12.5	9.3	10.1	17.8	12.1	20.5
8	10.6	9.8	12.3	12.4	11.4	14.4	8.9	14.2	14.2	15.0	15.3	12.5	11.7	11.7	12.4	15.1
9	9.7	10.6	12.8	13.9	15.5	7.1	10.4	11.3	12.9	10.5	14.5	9.8	10.9	13.6	11.7	18.6
10	7.9	10.3	14.7	12.8	11.2	10.2	8.7	12.3	16.3	13.1	14.9	9.5	11.9	19.7	12.4	24.0
11	9.1	11.5	13.9	15.0	10.1	9.3	11.7	12.8	14.3	10.9	12.0	11.1	13.5	13.2	12.0	14.7
12	7.9	10.9	9.8	15.3	13.1	7.0	11.4	14.3	16.2	14.4	11.6	9.9	13.5	14.9	12.2	22.2

fested a considerable amount of improvement.[2] One subject reduced his average error from 22 mm in the initial series to 7 mm in the final one. It will be noted that the individual performance from day to day was, in most instances, quite variable. Since external conditions were relatively constant, these fluctuations in accuracy must have been due to some

[2] Renshaw (5) also found for tactual-kinesthetic localization that some subjects failed to improve while others manifested marked improvement.

uncontrolled factor or factors within the individuals. The coefficients of variation indicate no consistent trend in the variability of the group averages from day to day.

The general trend of the group data is shown in Fig. 2. It will be observed, especially, that there is a sharp decrease

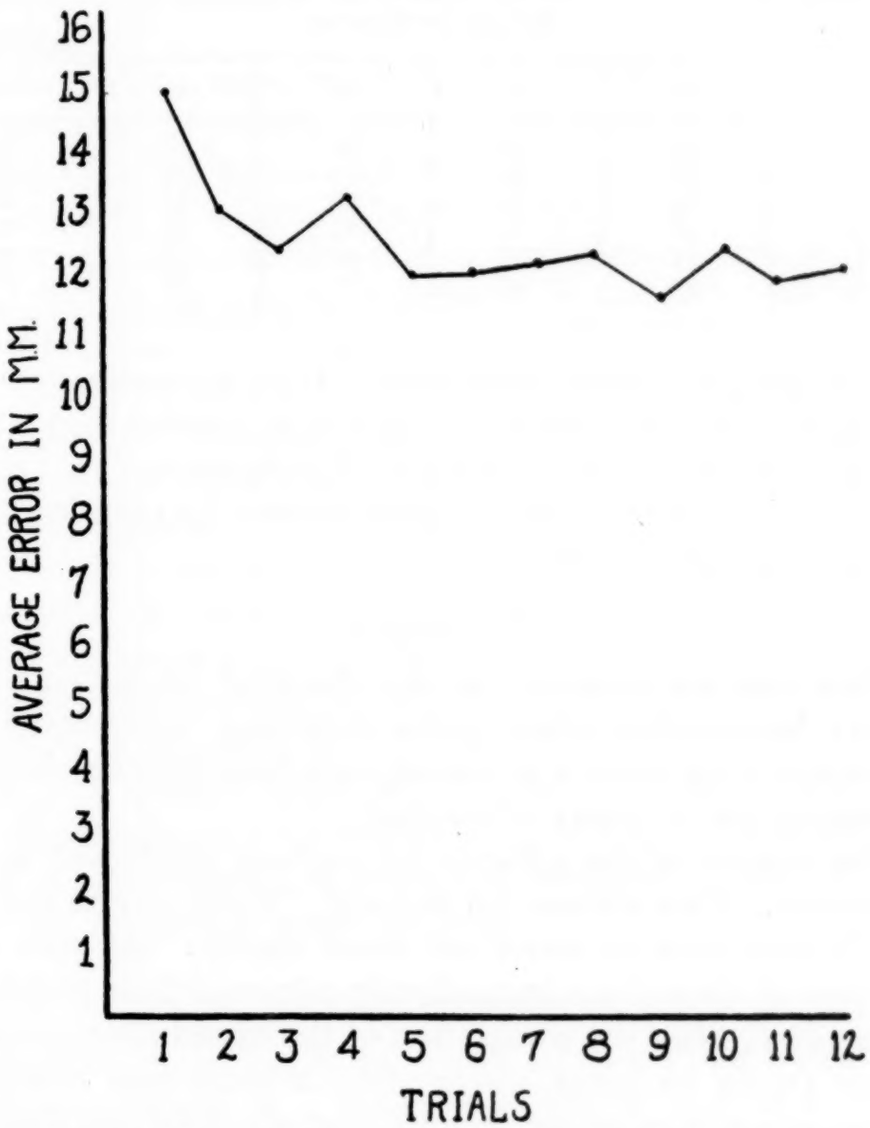

FIG. 2. Curve based upon the average error of localization in successive daily series of thirty stimulations.

in error during the first three days. This is followed by absence of any significant further improvement. A similar trend is evident when a curve representing the average number of correct localizations per day is plotted.

The subjects reported that they were much more confident

of the accuracy of localizations toward the elbow than toward the wrist and that the corner positions on the diagram were localized with the greatest confidence. The data in Table 2

TABLE 2

NUMBER OF CORRECT LOCALIZATIONS, OUT OF A POSSIBLE 168, FOR EACH SQUARE STIMULATED

	A	B	C	D	E
1	34	33	24	20	34
2	33	34	32	26	20
3	23	24	28	25	16
4	36	19	25	23	23
5	44	39	40	29	39
6	90	58	61	62	72

tend to support these judgments. It is apparent that the sixth row, which was nearest to the elbow, yielded the greatest number of correct localizations. Furthermore, the corner positions tended to have a higher correct localization score than did the inside ones.

DISCUSSION

How can we account for the observed improvement in tactual localization when overt localizing movements are absent and when there is no information from the experimenter concerning the accuracy of results?

The reports of the subjects are perhaps significant in this connection. One subject (C. B.) said, "From day to day the stimulations seem to stand out more clearly. As soon as I feel some of them I am immediately aware of their position." When asked, after the completion of the experiment, whether he was trying to judge where given stimulations would be with reference to previous stimulations which he had believed to be correct, this subject replied that he had purposely avoided such judgments. Most of the other subjects, however, gave reports similar to that of P. M. who said, "I know some of the corner positions and the middle ones along the outer columns and rows. I judge the position of other stimulations by asking myself whether they are to the right or left of C-1, for example." All of the subjects reported that

'felt' differences in stimulation were apparent from the beginning but that these could not at first be located within the frame of reference used in the experiment. As the experiment progressed, however, some of these 'felt' differences appeared to develop rather definite characteristics which led the subject to place them with respect to the diagram.

Although the experimenter gave no information concerning the accuracy of results, most of the subjects reported that certain types of inaccuracy were often apparent to them. For example, if a subject reported A-6 as the location of a given stimulation and later felt a stimulation closer to the elbow or farther to the left, he was aware of the fact that his A-6 report had been incorrect. Likewise, stimulation in, say, D-3 may have been reported as E-3. Should the next stimulation be farther to the right (*i.e.*, actually in the E column) the subject would know that the former one could not have been in E-3 as reported.

Thus this experiment does not involve learning without knowledge of results. A somewhat limited knowledge of results was present almost from the beginning. The limited nature of this knowledge perhaps accounts for the fact that accuracy of localization fails to improve after the first few trials. In tactual-kinesthetic localization (**5**), where there is a greater implicit knowledge of results and where improvement may be merely in localizing movements, the accuracy does not so quickly reach its limit.

Conclusion

Improvement in tactual localization without overt localizing movements of a differential nature has been demonstrated. Such improvement is associated with an increased knowledge of the sensory topography of the skin. Subjects report that *felt* differences in stimulation at various points or the skin acquire increased spatial significance as a result of practice. An implicit knowledge of results is derived from the relative positions of the points stimulated and this appears to form the basis of learning.

These results thus lend support to Peterson's view that the spatial characteristics of tactual stimulation may be developed in the course of adaptive behavior. The data of the present experiment also indicate that such development is not dependent upon overt localizing movements. The adaptive behavior may be symbolic in nature.

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THE EYELID REFLEX AS A CRITERION OF OCULAR FATIGUE

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The usefulness of an adequate criterion of ocular fatigue as one means of determining optimum conditions for easy seeing is obvious and such criteria have long been sought. Numerous attempts have been made to detect and to appraise the degree of ocular fatigue by measurements of visual function such as, for example, the measurement of visibility by means of the Luckiesh-Moss Visibility Meter (1) at frequent intervals during a period of reading. Notwithstanding the fact that the reading was continued in this experiment for more than two hours, no significant changes in the visual abilities of the subjects were observed. Furthermore, extensive researches indicate that the decrement in visual acuity, resulting from a period of eight hours of critical near-vision work, is of the order of two or three percent (2, 3). Obviously, such a differential in visual function is of no practical use in relating ocular fatigue with the visual conditions under which the critical seeing was performed.

If the brain is as hard a task-master for the eyes as it is known to be for the heart, the failure to detect ocular fatigue by measurements of visual function is easily understandable. As stated by Starling (4) "even when the heart in consequence to disease is scarcely able to carry on the circulation, the arterial pressure (carotid) undergoes little or no alteration. Any other tissue of the body, even the heart itself, may suffer, but the brain at all costs must receive its proper supply of blood." It is conceivable, *a priori*, that the brain likewise demands clear vision even though the ocular mechanism has been considerably fatigued by prolonged critical visual effort. This possibility is at least in harmony with certain observed facts. As has been stated, no statistically significant differ-

ence in visual function was noted as a result of continuous reading for two hours although a decrement in convergence reserve (adduction) of over 12 percent in prism diopters was observed after reading only one hour under similar conditions (**5**).

The conclusions of Ponder and Kennedy (**6**) that the "rate of blinking is closely related to the mental tension of the subject at the time and that the movements constitute a kind of relief mechanism whereby nervous energy, otherwise unutilized, passes into a highly facilitated path," suggests the possibility that this phenomenon may serve as a measure of human energy expended in critical seeing. In fact, Litinsky (**7**) has proposed the recording of winking as a method of study of ocular fatigue in children resulting from reading. Ponder and Kennedy also concluded that the eyelid movements are centrally originated and controlled, although they may be influenced along certain secondary paths, such as the second, fifth and eighth nerves. The authors have shown that the performance of critical visual tasks produces certain reflex effects which are considered to be of central origin and which may be correlated with the expenditure of human energy in seeing. In general, our experimental results indicate (1) that nervous muscular tension, developed as a result of continuous reading for a period of 30 minutes, decreases as the task of reading is made easier (**8**); and (2) that the heart-rate progressively decreases as the duration of the visual tasks increases and that this decrement in the heart-rate is also increased when the reading is made more difficult (**9**). Hence it may be supposed that eyelid movements, since they are supposed to be centrally controlled, may also be altered by 'reverberations' in the central nervous system and hence correlated with factors pertaining to the expenditure of human energy in seeing.

We have investigated this possibility directly with many subjects by observing the rate of blinking under the following experimental conditions: (1) during the first and last five minutes, respectively, of an hour of continuous reading, (2) during reading types of different sizes, (3) during reading

with and without a glare-source within the visual field, (4) during the performance of a visual task requiring rapid and alternate fixation of two different test-objects, and (5) during reading while wearing various ophthalmic corrections. Our data were obtained by visual observation and in accordance with the technique described in detail by Peterson and Allison (**10**). The experimental results are summarized in Table I. The detailed results are presented in Table II.

TABLE I

RATE OF BLINKING WHILE PERFORMING VARIOUS TASKS UNDER VARIOUS VISUAL CONDITIONS

In the computation of the probable errors presented, individual differences among the subjects were excluded.

	Visual Situation	Relative Rates of Blinking	
A.	Reading for one hour under 100 footcandles.[1]	First 5 min.	100
		Last 5 min.	146±10
	Reading for one hour under 1 footcandle.[1a]	First 5 min.	100
		Last 5 min.	195
B.	Performing a visual task requiring rapid alternate fixation of test-objects separated 30 degrees.[2]	First 5 min.	100
		Second 5 min.	146±6
		Third 5 min.	171±9
C.	Reading types of different sizes under 10 footcandles.[3]	12-pt. type	100
		6-pt. type	148±4
D.	Reading with and without glare. Glare-source (25 L.) at 20 degrees with line of vision.[3]	Without glare	100
		With glare	156±4
E.	Reading while wearing various ophthalmic lenses in addition to regular eyeglasses (when worn).[2]	+½ d. spheres	152±6
		Plano lenses	100
		−½ d. spheres	145±6

[1] Five subjects; 8 series of measurements.

[1a] Five subjects; 2 series of measurements.

[2] 18 subjects; 1 series of measurements.

[3] 18 subjects; 2 series of measurements.

DISCUSSION OF RESULTS

In general, it may be concluded from the data of Table I that the rate of blinking is increased without exception when the *specific* visual task is made more difficult or more prolonged. However, this generalization may not be extended

to include various *types* of visual tasks. For example, the visual task required in Test B (rapid alternate fixation) is undoubtedly more severe than that required in Test C (reading 12-point type) yet the rate of blinking is slower in the latter as will be noted from Table II.

TABLE II

THE VALUES GIVEN ARE THE NUMBER OF BLINKS OCCURRING IN FIVE-MINUTE PERIODS

Subject	A. Hour of Reading Periods (5 min.)		B. Alternate Fixation Period			C. Type Size		D. Glare		E. Corrections		
	Before	After	1st	2nd	3rd	6-pt.	12-pt.	With	Without	+ .50 d	0	− .50 d
1	26	30	23	24	27	46	32	25	19	41	29	46
2*	18	32	11	18	24	40	28	26	18	40	31	39
3*	32	56	17	24	24	47	30	32	19	21	22	25
4*	24	31	41	56	72	56	35	40	30	73	58	70
5	12	16	10	14	17	30	20	52	28	28	17	21
6			15	24	28	70	50	107	80	80	55	80
7			11	10	13	18	14	12	6	26	19	24
8			18	23	26	26	16	22	18	29	15	22
9			30	37	41	22	16	32	19	40	20	33
10			12	17	17	38	28	19	12	57	39	67
11			5	10	13	18	8	20	10	8	11	13
12			6	7	8	10	10	18	14	21	11	22
13			3	7	10	33	26	56	34	29	18	18
14*			13	24	31	22	18	17	11	15	8	16
15*			33	34	26	52	42	50	33	38	29	42
16*			9	11	11	10	6	11	5	11	8	13
17*			19	27	33	31	25	52	38	60	36	30
18*			8	16	15	28	18	32	22	28	14	27
Arith. mean	22.4	33.0	15.8	21.3	24.2	33.2	23.4	34.4	23.1	35.8	24.4	33.8
Av. variation	4.1	9.2	7.8	9.1	10.5	13.0	10.0	11.2	6.0	15.8	11.8	15.7
Probable error	1.7	3.9	1.6	1.9	2.2	2.7	2.0	2.3	1.2	3.2	2.4	3.2

* Correction worn.

Test A.—Since it is axiomatic that fatigue increases with the duration of the task, it follows from the results of Test A and Test B, Table I, that the rate of blinking is a function of the duration of the task and a measure of the fatigue induced by performing the task. Without exception, the subjects blinked at a faster rate at the end of the test-period than at the beginning. In Test A an effort was made to provide physical conditions favorable for easy reading, and in addition, the subjects were allowed to select books which interested

them. Under such conditions the task of reading for an hour was not a difficult one when appraised introspectively. However, large increases in the rate of blinking were observed.

Test B.—The performance of this test involved severe ocular muscular effort of a character described in detail elsewhere (**11**). The subject had to look back and forth from one field to another and to distinguish a letter which appeared in each field. The letters were changed with each alternation. It will be noted that the average rate of blinking during the second five-minute period increased 46 percent over that of the first five-minute period or by an amount equal to that resulting from an hour of reading. During the third five-minute period, the rate of blinking was 71 percent faster than it was during the first five-minute period.

However, it will be noted from the data of Table II, that the number of blinks during the first period of Test B is definitely less than in the experiments involving reading. Since the lateral displacement of the point of fixation requires a voluntary innervation of the third nerve which also supplies the levator palpebræ superioris, it is to be expected that the antagonistic orbicularis palpebrarum would reflexly be inhibited resulting in an initial lowered rate of blinking. It is also conceivable that the observed decrease in the rate of blinking is due to the fact that excessive blinking interferes with the performance of this rapid and exacting visual task. In any event, the criterion of eyelid response is applicable for appraising the relative difficulty of various visual tasks only after allowance is made for any change in the base rate of blinking due to such reflex effects. The experimental results of Telford and Thompson (**12**), Table III, apparently corrobo-

TABLE III

THE MEAN NUMBER OF EYE-WINKS PER FIVE-MINUTE PERIOD AS OBTAINED FROM 36 SUBJECTS

Occupation	Number of Blinks	Standard Error
Mental Arithmetic	54.4	4.0
Reading	14.4	2.7
Conversation	44.5	1.6

rate this conclusion in a qualitative manner. Telford and Thompson suggest that the decreased winking found during reading is not due to the mental activity involved, but possibly to several other factors such as the visual fixation involved in reading, the eye movements, and other variables. The latter may either increase or decrease the so-called normal rate of blinking.

Test C.—Since the fixation and perception of small objects is known to be more difficult than that of larger objects, this phase of visual difficulty was investigated by requiring the subjects to read matter printed in 6-point and 12-point types, respectively. The test-periods were five minutes in duration and so arranged that errors due to the order of the tests were eliminated. The number of blinks while reading the 6-point type was 48 percent greater than for the 12-point type, with a corresponding probable error of 4 percent. These data, indicating that the reading of 6-point type is much more difficult than reading 12-point type, are in harmony with the empirical fact that 6-point type is generally regarded as unsatisfactory for printed matter which is read for rather prolonged periods.

Test D.—It has been demonstrated that a glare-source within the visual field augments the development of nervous muscular tension (**13**) incident to the performance of a critical visual task. In view of the data already presented, it is to be expected that such a situation would also increase the rate of blinking. The data of Test D verify this supposition. In this experiment a 50-watt inside frosted lamp was placed at a distance of 1 meter from the eyes of the subject and at an angle of 20 degrees with the line of vision while reading. This glare-source, having a brightness of approximately 25 lamberts, was more annoying than uncomfortable according to the introspective reports of the subjects. Thus the glare-source is considered as a distraction whose influence is due to the inherent tendency of the human eye to fixate a bright peripheral object. The number of blinks with and without the glare-source was 34.4 and 23.1, respectively, during the five-minute period with corresponding probable errors of

2.3 and 1.2. The conclusion that the increase in the rate of blinking is due to the distracting influence of the glare-source rather than to direct photic stimulation is in agreement with a statement of Ponder and Kennedy to the effect that a sudden illumination of the eyes, as by a bright light concentrated upon them, may produce a few rapid blinks, but after a very short time the rate falls to about the normal for the individual.

Test E.—The criterion of the reflex blink was also applied to the appraisal of the effects of refractive errors in the optical system of the eye. The number of blinks during a five-minute period of reading was counted with the subject wearing (1) plus ½ diopter spheres, (2) plano lenses and (3) minus ½ diopter spheres in addition to his usual correction, if any. These additional corrections resulted in either blurring the retinal image or dissociating the normal relationship between accommodation and convergence. It is obvious from the data of Table I (Test E) that the condition of maximum comfort or minimum strain and fatigue was obtained with the prescribed corrections as worn by the subjects as a group. This conclusion applies to the individual subjects with the exception of Subject 17, Table II. In the latter case, it will be noted that the minimum rate of blinking, for these experimental conditions, was obtained when minus ½ diopter spheres were added to the correction worn; and a refraction without mydriatic indicated that this subject needed approximately such an addition to his correction. An analysis of other individual results presented in Table II indicates that not all subjects were perfectly corrected if minimum rate of blinking be accepted as a criterion of the accuracy of the ophthalmic corrections. However, the errors are relatively small, and in view of the meagerness of the data, further quantitative interpretation is not warranted. Nevertheless, it seems reasonable to conclude that the reflex blink may prove eventually to be a significant diagnostic criterion in the science of refraction, particularly if the data were automatically recorded for rather extended periods. The authors have such an investigation and development in progress.

Summary

These data indicate that the rate of blinking while performing critical visual tasks is a function of both the duration and severity of the specific visual task; and that an augmentation of either of these factors increases the rate of blinking. It must be considered that the initial rate of blinking is not necessarily a measure of the relative amount of human energy expended in the performance of visual tasks of different characteristics and requirements, since reflex inhibition of blinking will also be involved.

The authors are indebted to Dr. E. Q. Adams of the Lamp Development Laboratory of the General Electric Company for many helpful suggestions.

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ILLUSORY TRENDS IN THE OBSERVATIONS OF BAR GRAPHS *

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Bar graphs are utilized to present a picture of some statistical data for rapid comprehension. The story of a bar graph is told in the perception of the relative linear estimates of the bars that it contains. These present a figure in some background, which is essentially an area bounded by a more or less sharply defined rectangle. The nature of this background is determined by various factors, many of which are arbitrarily decided, such as the size of the scale units, bar widths, spacing between the bars, shading, cross hatching, color or brightness of the component parts, and horizontal or vertical mode of presentation. By selection, the areas may be squares of different sizes or rectangles, varying from long slender ones presented vertically to short fat ones presented horizontally. The importance of conclusions derived from observations of such graphs seemed to justify an examination of the fidelity of these observations to mathematically measured lengths, and especially an examination of whether the same estimates would be made irrespective of background or the relative height of the bars.

The experimental investigation used 88 different bar graphs and attempted such simplification as the limitations of the complex task admitted. Factors studied that might influence divergence in observation and their varying degrees were as follows:

1. Nearness or remoteness from the scale. All graphs contained five bars, the longest a constant (100 percent),

* In the initial stages of this investigation, I am indebted to Dr. E. C. Bratt for aid in formulating the problem related to the differential influence of the vertical or horizontal mode of presentation and for aid in constructing and photographing some of the bar graphs used.

with four to be estimated. The bars were graduated in order of size; the average lengths, in terms of the constant or scale bar, were 77, 56, 37, and 20 percents respectively. Relatively longer bars were always nearer the scale bar.

2. Horizontal or vertical presentation of the graph bars.
3. The coarseness or fineness of the scale units. Four degrees were employed, 100, 83.33, 66.66, and 50 percents respectively. This factor determines one dimension of the enclosed rectangular area of the background.
4. The spacing between the bars in six degrees. In percents of the widest spacing used, these were 100, 66.66, 33.33, 16.66, 5.55 and 0.0
5. The width of the bars in four degrees. In percents of the widest used, these were 100, 50, 25, and 8.33. Bar widths with spacing distances between the bars determine the other dimension of the rectangular background area.
6. Length of the bars. (In general, bar lengths are determined by the statistical data plotted and are not subject to arbitrary manipulation.) In this study, when bar position as to nearness to the scale bar was held constant, 15 distinct difference values were used, ranging from zero difference to 27.5 percent of the background height or width, depending upon the mode of presentation.

The possible influence of such factors as lens distortion in photographing and projecting, common recordings in fives of percent, and roughness of measuring scales or chance variations in measurement were ignored. Readily readable units of the scale used in bar construction ranged from .005 to .01 of the total bar lengths for the longest estimate bar to .02 and .04 for the shortest.

In order to set up the bar graphs, five bar grids were cut out in a rectangular area of a number of dull black mats, the bars and openings of which were designed to correspond to the spacings and bar widths to be used. Glossy white strips of very thin cardboard were inserted between the grid

and an identical mat in each opening, measured for length by means of a scale and a drawing compass, secured in position with pins and the bars of the grid, and photographed using a copying lens. The focal distance was determined by the maximum utilization of the film, which would give a small margin to the left or bottom. The negative was used in projection, thus reversing the blacks and whites. The film and development used emphasized the contrast effects. Projection was made on a light protected wall screen in front of the class room. Estimations were secured and analyzed for 352 bars from each of 50 students registered in a course in Applied Psychology. Sharply contrasting boundary lines on the background area were omitted, but dim lines on the mat presumably aided in revealing limits.

The order of presentation of the variables in their varying degrees used the familiar A B B A technique to equalize practice, fatigue, and motivational effects, and to conceal recurring percentage patterns. The effectiveness of this concealment was checked by a questionnaire which indicated that no one was able to make conscious use of these recurring percentage patterns. An orderly arrangement in construction and presentation permitted an analysis both of the variables and their degrees.

Results and inferences are analyzed in the following tables and paragraphs:

I. *The Statistical Pattern.*—Every bar graph pictures frequencies, averages, or some linear aspect. The question raised was, "Is the nature of the observation influenced by the relative length of the bars determined by such data?" As a specific illustration of this, does it make any difference for two typical graphs, such as one having bar lengths of 100, 90, 60, 33.33 and 15 percent of the scale, and another having lengths of 100, 70, 45, 35, and 25 percents, whether bar 2 is 90 or 70 percent of its scale bar, or whether bar 5 is 15 or 25 percent? From such evidence as that portrayed in Tables I and II, a common illusory trend toward over-estimation of the shorter bars at any given bar position seems to be fairly convincing, except where the difference is less than 5 percent of the scale.

TABLE I

Percent Diff.	Freq.	Md. of D/σ_d's	Q	Prob.
0– 4 percent.....	5	−1.5	4.5	7
5– 9 percent.....	20	1.5	2.1	93
10–14 percent.....	13	3.8	.9	100
15–19 percent.....	9	4.0+	.6	100
20–24 percent.....	8	4.0+	.0	100
25–up percent.....	5	4.0+	.0	100

TABLE II

Width	Ave. Percent Diff.	Md. of D/σ_d's	Q	Prob.	N of 100 ea.
12	18.7	3.1	1.6	100	144
6	16.3	2.9	1.3	100	144
3	2.9	1.0	1.8	84	144
1	16.0	2.2	1.7	99	120
Bar					552
2	4.4	2.3	1.4	99	138
3	11.5	3.0	1.2	100	138
4	3.8	1.6	2.1	95	138
5	1.4	2.3	1.6	99	138

In the column headings above, 'Percent Diff.' and 'Ave. Percent Diff.' = the differences in bar lengths expressed in percents of the scale bars; 'Freq.' = frequencies in terms of paired groups, each of which was based upon an average of 675 observations. 'Md. of D/σ_d's' = the median of differences in averages divided by sigma of the difference—a positive sign indicating the reliability of the differences favoring over-estimation of the shorter bars, and a minus sign under-estimation. 'Q' = the semi-interquartile range of these differences in averages divided by the sigma of the difference. In these tables, the size of this measure reflects the weighted influence of unequal quantities in the non-identical combinations and cannot be considered solely as a measure of the dispersion due to different bar lengths. (See further explanation below.) 'Prob.' = the chances in 100 of the median direction holding consistently. 'Bar' indicates the position of the bar as to nearness or remoteness from the scale bar and, since they were always presented in graduated order as to length, it also indicates relative bar lengths within any graph, 2 being the longest estimated bar and 5 the shortest. 'Width' indicates numbers which express the relative widths of the bars in the various graphs. 'N' indicates the number of distributions used in determining the measures found in that particular row. Each distribution is based upon 100 observations—two graphs, which are identical in almost all particulars.

In order to clarify these tables and the inferences drawn from them, as well as from later tables, certain of my assumptions and methods of analysis are here included:

1. It was assumed, on the one hand, that, if no illusory influence was present, one should expect a normal distribution of estimates around the correct linear measurement; and, on the other hand, if such an illusory influence was present, that the distribution would cluster around some other point consistent with the direction and degree of that influence. (It was determined that the typical nature of the distributions of these estimates was Gaussian in type.)

2. A very simple linear unit of measure was used, namely, that one contrasted factor gives more over-estimation or less under-estimation than the other.

3. The inclusion of some six variables with a number of degrees for each, ranging from 2 to 15 would have required for identical comparisons save for the two controlled factors, the construction and estimation of millions of graphs by the pair comparisons method. Since this was impracticable, some sort of grouping was used. In one, a comparison was made of several graphs having identical combinations of both variables and their degrees; in the other, groups of graphs were analyzed, which were equated both for the combinations of factors and for the degree of the factors but not in identical combinations. These 'equated' groups were handled in two ways, by throwing all the estimations in the contrasted factors into two fairly large distributions and obtaining the differences in means divided by the sigma of the difference; or by comparing each member of one group with each of the other; these results could then be drawn on a sigma scale, using the cummulative frequency concepts, and computing medians and upper and lower quartiles. The scale used was -4σ, -3σ, -2σ, -1σ, -0σ, $+0\sigma$, $+1\sigma$, $+2\sigma$, $+3\sigma$, $+4\sigma$. Over-estimation was arbitrarily selected for one of the contrasted factors, with its under-estimation given negative values. The median of this ogive distribution of the D/σ_d's on such a scale was taken as a fairly representative measure of the typical influence of the factor or quantity as to the probability of maintaining the direction of the trend toward over-estimation or under-estimation consistently. For example, the comparison of ab_1cd_2 with ab_2ed_1 would not be comparable when the letters represented different variables and the numbers represented different quantities because of the differences in the quantities of b and d. Yet $a_1b_1cd_2 + a_2b_2cd_1$ compared with $a_1b_2ed_1 + a_2b_1ed_2$ might be meaningful for revealing the median influence of c compared with that of e. In Table I such groupings are thrown into one distribution for c and one for e, D/σ_d's computed, and grouped on a sigma scale, the median of which is given in the table; while in Table II, the distributions include only one or two graphs which were practically identical; each distribution was then compared with every other in the more broadly equated group and cast on the sigma scale, the median of the group then being recorded in the table.

In nearly every comparison in tables I and II, it was possible to hold identical one or more variables, while others could be equated for factors and quantities though not in identical combinations. Bar position, for example, was kept identical throughout. In Table II, about $\frac{1}{5}$th of the variables and their degrees could be identically grouped. 92 percent of these included differences greater than 2 percent, were all found to be favorable to over-estimation of the shorter bars, and were statistically reliable with critical ratios as high as 15. Since the problem was set up essentially to study background variables, it turned out that approximately three-fourths of the statistical differences in Table II for width 3 were small, averaging about 2 percent. Here it is noted that the probabilities of over-estimation favoring the shortest bars are least. If the assumptions made are tenable, then the two tables above favor the conclusion that the shorter bar is, on the average, likely to be over-estimated for percentage differences of 5 percent of the scale or over.

Let us digress for a comment upon the question of the ability to predict from trends found in illusions in simple situations to those found in such complex situations as bar graphs. Such predictions need verification. For example, take the common filled-unfilled space illusion. In the simple

situation, where distances *AB* and *CD* are equal in length, if *AB* is filled as in the case of a line or a succession of dots, and *CD* unfilled, *AB* would typically be judged as longer. In the bar graph situation, we find several bars presented in a rectangular area. In a sense, a bar extends from one boundary to some point within the area, the remaining distance to the opposite boundary being unfilled space. If *AB* again represents the filled and *CD* the unfilled space, logically *AB* should be over-estimated. The following facts might be noted from the analysis of the estimations. The means of 4400 estimations for each estimated bar are in the direction of over-estimation. Thus far the two situations are comparable. However, the longer the bar, the less the over-estimation. It is difficult to explain why the shortest bar gives a mean over-estimation that is 55 times that for the longest bar. I believe that the inferred contradiction can be explained by a difference in the mode of response. In the simpler situation, the comparison is directly of the relation between the filled and unfilled spaces, while in the graph situation, verbal reports indicate that subjects typically react to differences and subtract to obtain the desired bar lengths. If the greater over-estimation of filled to unfilled increased their apparent length of the bars, one short and one long, the smaller unit subtracted from the greater would in the case of the long bar be an unfilled shrunken unit, and in the short bar's case a filled exaggerated unit. Thus assuming the correctness of the filled-unfilled illusory tendency, a change in mode of responding could account for the greater over-estimation of the relatively shorter bar. Even a direct comparison of filled space with filled space, or bar with bar, would be influenced by such considerations, unless the background area, sharply defined or imaginatively projected, could be eliminated.

II. *The Coarseness or Fineness of the Scale Units.*—The arbitrary selection of the size of the scale units influences one dimension of the background area. Does this selection make any real difference in the nature of the observation? The analysis in this study did not permit identically equating

the groups so that all factors which would affect the medians were entirely controlled. However, the analysis was so arranged that the two unequated factors, namely, mode of presentation and statistical pattern, would react adversely against the common trend. Without correcting for these adverse trends, there is evidenced a slight unreliable trend toward over-estimation of the relatively coarser units. Tables III[a] and III[b] present these results:

TABLE III[a]

Ratio	Md. D/σ_d	Q	Prob.
1 : 2	2.0	2.2	98
2 : 3	2.0	1.0	98
5 : 6	1.5	2.5	93
3 : 4	0.0	1.2	50
4 : 5	0.0	1.7	50
3 : 5	−0.5	2.7	31

TABLE III[b]

Ratio	Md. D/σ_d	Q	Prob.
1 : 2	1.5	1.3	93
4 : 5	2.0	.7	98

'Ratio' = the ratio of one scale unit to that of the other one compared. 'Md. D/σ_d' = the median of the D/σ_d's arranged on a cummulative scale. 'Q' = the semi-interquartile range of the D/σ_d's. 'Prob.' = the chances in 100 of the median direction holding consistently. A minus sign indicates a median trend in the direction of the under-estimation of relatively coarser units and a positive sign that the direction is toward its over-estimation. The assumptions and method of treatment previously described were employed. The results in Table III[a] are based on grouping which combine distributions into a number of fairly large ones. About 13,000 observations on each of the four scale units were available for Table III[a]. Groups in Table III[b] were narrowed to include combinations identical as to bar and width, and the two bars least influenced by mode of presentation. The influence of the statistical patterns affects the median adversely to the common trend to a considerable degree. The medians are based upon a total of 2400 and 2200 observations respectively.

III. The Variation of the Width of the Graph's Bars.—This factor along with the amount of spacing between the bars influences the size of the other linear measure of the background area. Does the relative width of these bars make any real difference in the interpretation of the data of the graph?

In the broad groupings used in the analysis, all factors save that of the statistical pattern were approximately controlled. These patterns influence the medians in a direction contrary to the common trend, and exert a marked influence on the size of Q. There is a slight suggestion of a tendency to over-estimate the relatively narrower bar widths.

TABLE IV

Widths	Md. D/σ_d	Q	Prob.
100 : 50	1.0	3.2	84
100 : 25	3.0	2.2	100
100 : 8.3	3.0	1.0	100
50 : 25	3.5	3.5	100
50 : 8.3	1.5	2.0	93
25 : 8.3	−1.0	2.5	16

TABLE V

Widths	Md. D/σ_d	Q	Prob.
100 : 50	.8	2.1	78
100 : 25	1.3	1.9	90
100 : 8.3	.3	1.2	62
50 : 25	0.0	2.1	50
50 : 8.3	.2	2.1	56
25 : 8.3	1.4	1.9	92

In the column headings, 'Widths' indicate the widths in percents of the widest bars used. 'Md. D/σ_d' = the median of the D/σ_d's. Positive sign indicates over-estimation of the narrower bars and a minus under-estimation. 'Q' = the semi-interquartile range of the D/σ_d's. 'Prob.' indicates the chances in 100 of the median direction holding consistently. In Table IV, the mode of grouping combined the equated groups into a single distribution. The rows average in this way observations of about 1075 cases each. In Table V, each row represents the median of 8 groups, compared by a paired comparisons method with each of the other members of the contrasting group. Hence, each row is based upon an average of 800 observations.

IV. The Variation in Spacing Distance between the Bars.— Does the selection of the spacing distances between the bars make any real difference in the observation of bar lengths? An analysis was attempted by equating in broad groupings the various factors and their degrees. This analysis suggests that probably there is some tendency to over-estimate the wider spacing; this is comparable to the trend found for the over-estimation of coarser scale units, if area is considered.

However, such a generalization is too broad since the probability of some trend toward over-estimation of the narrower spacing has been noted. Also, larger critical ratios for spacing distances were obtained, if finer units of measure were used than if coarser, as can be noted in the table below, when comparable ratios but non-comparable units are compared.

TABLE VI

Space Dist.	Md. of D/σ_d's	Q	Prob.
100 : 67	0.0	1.3	50
100 : 33	.5	1.7	69
100 : 17	.3	.6	62
100 : 5.5	3.0	1.5	100
100 : 0	4.0+	1.1	100
67 : 33	− .5	2.2	31
67 : 17	− .5	1.5	31
67 : 5	2.0	1.0	98
67 : 0	3.5	1.5	100
33 : 17	0.0	1.7	50
33 : 5	3.0	.7	100
33 : 0	3.7	.4	100
17 : 5	4.0+	1.0	100
17 : 0	3.5	1.0	100
5 : 0	1.0	1.5	84

TABLE VII

Space Dist.	Md. of D/σ_d's	Q	Prob.
66 : 33	3.0	.8	100
100 : 17	1.7	3.2	96

'Space Dist.' indicates the spacing distances between the bars in percents of the widest bars. 100 percent in this case is 50 percent wider than the 100 percent for bar widths. 'Md. of D/σ_d's' = the median of the D/σ_d's. A positive sign indicates over-estimation of relatively wider spacing distances, and a minus under-estimation. 'Q' = the semi-interquartile range of the D/σ_d's. 'Prob.' = the chances in 100 of the median direction holding consistently. In Table VI, the groups were thrown into single distributions for comparisons, and 120 such pairs were compared in Table VI. In Table VII, 32 pairs are reported, based upon distributions of 100 each. In this table, the statistical patterns and mode of presentation are not equated but in the two rows they are exactly reversed as to the influence of these factors.

V. The Vertical or Horizontal Mode of Presentation.— Does it make any difference whether we present the graph vertically or horizontally? The analysis of the observations was approximately equated save for scale units, and the

grouping was made to have the scale units influence the medians in a direction counter to the over-estimation of the vertical. In spite of this adverse influence, a slight trend toward over-estimation of the vertical presentation is indicated. This trend is reliable statistically for the longest bar. Logically, it is in the direction predictable from knowledge of the vertical-horizontal illusion in simple situations. Yet, in the case of the shortest bar, this direction is reversed to favor over-estimation of the horizontal presentation, though not reliably so. This, combined with some observations made in a short study on the estimation of volumes, raises the question of the necessity of determining the critical limits within which such illusory trends may hold. Logically, it might be inferred from certain information deduced from a study of the vertical-horizontal illusion in simpler situations and from Helmholtz's suggestion on their explanation as due to confusion derived from the habit of using vertical lines as cues for perspective, that a longer bar would be influenced more than a shorter bar toward over-estimation of the vertical presentation. A unit of length in the upper portion of a bar or line does suggest more length than that in the lower portion. This influence is commonly known and may by predispositions and habits become compensated for, consciously or unconsciously, in the development of skill in observing; hence,

TABLE VIII

Bar	D/σ_d	Prob.	Ave. Length	Low	High
2	3.5	100	77%	65%	90%
3	2.1	98	56%	40%	75%
4	.8	79	38%	25%	50%
5	−1.6	6	20%	10%	30%

'Bar' indicates the position of the bar on the graph. 2 indicates that it was both the longest estimated bar length on any one graph and next adjacent to the scale bar. 5 at the other extreme indicates that it was both the shortest bar on the graph and most remote bar from the scale. 'D/σ_d' = the D/σ_d's of the compared groups. 'Prob.' = the chances in 100 of the direction of the trend holding consistently. 'Ave. Length' indicates the mean of the bar lengths in any one bar position in terms of the percents of the scale bars. 'Low and High' indicate the extreme range in bar lengths in any one bar position in terms of the percents of the scale bars. The number of observations in any row was 2150 in the vertical group and 2200 in the horizontal.

where the illusory effects are weakest, even over-compensation may take place. Table VIII shows some analysis of this question of the effect of the mode of presentation.

VI. Nearness or Remoteness from the Scale.—Does the position of the bar relative to the scale influence the nature of the observation of its length? In the graphs constructed, the longer length was inseparable from nearness and the shorter length from remoteness. A tendency was found for the relatively shorter, more remote bars to be over-estimated. Also, with one exception, the size of the standard deviations increased with remoteness and shortness. In comparing the bar nearest the scale with the two most remote, the critical ratio for the most remote was 17.3 and for the next most remote was 10.6.

Summary and Comment.—This study presents the direction of the influence based upon the averages of the two contrasting factors, and the probability of this direction holding consistently based upon the size of the difference in averages divided by the sigma of the difference. These factors which should give the maximum amount of over-estimation in combination are short bars, presented with wide spacing distances, coarse scale units, and narrow bars viewed vertically if the most important comparison is a long bar, and horizontally if it is short. The bar positions remote from the scale, also, give greater over-estimation. Several of these factors in combination did seem to afford some explanation for certain large critical ratios.

The magnitude of the illusory trends has not been included in this study. One reason for this is that the problem was not adequately set up for the determination of precise amounts of influence. Even if this were no objection, I believe that one should be exceedingly cautious about applying constants to correct observations of this sort with the expectation of getting fairly reliable observations. Caution is especially needed where observational skill is shown to be poor. Situations involving observational judgments may often be checked by some other system, such as by mathematical measurement or computation, whereby marked discrepancies can be shown

when the two modes are compared. On this matter of the judgment of a bar's length, the range of skill is indicated by the fact that the standard deviations for the shortest bar ranged from 6.5 percent to almost 50 percent of the bar's length. Furthermore, in many such situations, skills are likely to remain poor, because neither the incentives nor habits of checking promptly the accuracy of observations are adequate to develop skill. How meaningful, then, can an average for individuals at an unskilled stage of the learning curve be, if that average is designed to obtain a precise correction constant for one's observations? Also, how representative can such a group be for the determination of such a constant? However, illusions, which are merely incorrect perceptions habitually fairly constant in direction, seem to indicate that for the matter of direction such a group may be fairly representative and typical of other groups for the over or under estimation of their judgments. Hence, it may be worthwhile to investigate the typical direction of illusory habits of response in order to call attention to the inaccuracies of observation, to common trends as a corrective aid or to the need to improve skills. The essential task is not, however, that of finding a compensating formula but one of learning. That one need not be bound to low observational performance because of common illusory habits is attested by the fact that fishermen do become expert in spearing fish and eels under water in spite of the common illusion in the situation.

The importance of this study probably lies in the fact that it shows that typical observations may vary with various modifications in the construction of the graphs, that it indicates the direction of illusory tendencies for certain qualifying factors, and that it stresses the need for verifying inferences from simple illusions, and for checking and learning, if skill in estimating bars is to be acquired for varying kinds of graph construction.

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ILLUSORY TRENDS IN THE OBSERVATIONS OF VOLUMES

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The illusory trends in such complex situations as that of observations of volume, have received very little consideration, though illusions in simple situations have been subjected to much experimental investigation. In the past, the primary interest behind illusion studies has been to throw light upon perceptive processes. In addition, the experimenters have preferred to study illusions in simple situations, in which the variables were reduced to a minimum, and, if possible, controlled by mechanical restrictions, and aided by mechanical recording. However, other experimenters prefer to take situations, actual or simulated, as they find them, and introduce only such degree of simplification, control, and recording aids as this approach admits. They feel that deductions drawn from the controlled and simplified situations of the laboratory need verification in the complex situations before they are entirely acceptable, and that assumptions regarding the direction and degree of the influence of a variable require checking. The usual failure to combine these complementary attitudes in one scientist, encourages the controversy over which method is the more desirable, and fosters criticisms of the practical minded investigator for the pure laboratory method, and of the experimentalist for the weak controls of the naturalistic approach. The need for both methods is attested by those engineering problems which require both the simplified researches in the laboratory and the development of models [1] for simulating the conditions to be met in the complex situations.

A preliminary study of the volume situation indicated the

[1] Rose, M. A., *Trial by Models, Today*, June 6, 1936.

need for verifying the inferences derived from simple situations before utilizing them directly in complex situations.

Under certain conditions, a line presented vertically tends to portray greater length than if presented horizontally. I raised the question whether one could infer that the height of a cylinder tended to influence over-estimation more than a corresponding diameter. I took a solid wooden cylinder having a diameter to height ratio of 1 : 1 and compared it with another cylinder having a diameter ⅛ less than the 1 : 1 cylinder, and ⅛ greater height. All of the 12 subjects used over-estimated the wider diameter (1 : 1 cylinder). The median over-estimation was 15 percent of the volume of the 1 : 1 cylinder. In 23 other comparisons of pairs of cylinders, with diameter to height ratios ranging from 1 : 1 to a little over 1 : 2, 20 medians were definitely more favorable for over-estimation of diameters. However, in the later investigation on more subjects, I used fewer cylinders and kept heights and diameters equal in the series and expected to find that diameters were over-estimated by differences that would be significantly greater than for heights. This I failed to find. Both tended to be over-estimated when the other was held constant, though owing to the poor skill of my subjects, the question of which was relatively more over-estimated was not answered. However, taken with the preliminary trials, a better case can be made for the over-estimation of diameters than for heights. This trend runs counter to that which might logically be expected from recalling the familiar vertical-horizontal illusion.

The volume study secured about 87 different volume judgments from four groups of approximately 20 students at a time. Both subjective and objective comparisons were required. An illusion was considered to be merely an incorrect observation, probably made at a relatively unskilled stage of the learning curve, but habitually fairly constant in direction. Hence, an illusory tendency was felt to be shown by the fact that a typical distribution of observations clustered around some point other than that mathematically computed measure by which its inaccuracy was revealed. In addition

to the question just discussed of whether or not one is justified in predicting an habitual trend by logical inference from that of lines to that of volumes, a number of other questions were raised as problems for investigation.

1. Are there fairly constant illusory trends in the estimation of the capacity of containers holding the equivalents of familiar liquid measures?

2. Are there habitual differences in the direction or degree of such trends that vary with the form of the solid or container?

3. Are there fairly consistent habitual trends which are explainable as due to certain linear influences found in the volume situation?

4. Supposing such linear trends are found to be influential within some narrow or moderate range, can they be expected to hold in extreme situations? In other words, are there critical limits to their applicability?

Question: 1.—Are there fairly constant habitual trends in the estimation of volume equivalents of familiar measures? The following measures were studied: the ounce, ½ pint, pint, quart, and gallon. Every judgment was based upon the student's remembered volume of these measures. Before making his judgments, the student had presented to him a solid form which had the exact relative measurements of those required. He was asked to state one linear dimension in inches that would provide a volume the exact equivalent of the familiar measure. The forms used and the linear dimension required were heights for cubes, for cylinders having ratios of diameter to height of 1 : 1 and 1 : 2, and for rectangular solids having ratios of non-parallel sides of 1 : 2 : 4, which rested on a base 1 : 2; and diameters for globes. Table 1 presents the data in a form which grouped the five forms by averaging. Since the trend noted was uniform in direction in all forms, this grouping reduces tabular size and affords approximately 350 observations for each measure. It is to be noted that the relatively small measures are under-estimated and the large over-estimated. Possibly, if measures are considered large there is a common habit of over-com-

pensating toward over-estimation, and, if small, toward under-estimation. This explanation perhaps needs elaboration.

TABLE 1

Measure	Diff. between Md. and Math.	Q	Ave. Percent Overlap
Ounce	−55	28	18
½ pint	−49	80	31
Pint	+87	143	69
Quart	+217	330	86
Gallon	+344	1100	82

Column 2 shows the difference between the median estimation and the mathematically computed volume in percentages of the volume of the measures used. Column 3 shows the semi-interquartile range in similiar percentages of the measured volumes. Column 4 indicated the percentage of individuals reaching or exceeding the measured volumes. Each row is based upon approximately 350 cases.

We might observe that the situations in which we find illusions are those in which we are relatively unskilled or confused. Under such conditions, a preliminary suggestion, adjustment, or predisposition, may modify the response markedly. If we, then, entertain the suggestion that this is a small volume, our response is apt to be too small. Let us take an example in the simpler field, namely, the size-weight illusion. Suppose we have a series of graduated weights uniform in size from 5 to 100 grams, and two weights for estimation, one is large—a block of wood hollowed out, the other is small, a block of wood loaded with lead. Both these weights weigh exactly 55 grams. Adults commonly have developed a sense of the weight of wood blocks, and exercise that memory in the adjustment made to lift these blocks. If the adjustment is greater than needed to lift the large block and less than needed to raise the small block, lightness is suggested for the large and heaviness for the small. In the response made in matching the large block with the set of uniform weights, over-compensation is made for lightness and the large block is markedly under-estimated, and similarly the small block is over-estimated. Young children, interestingly enough, reverse the direction of the illusion, but the explanation need not change. They have developed no adequate

kinesthetic memory but have a memory for the relationship between size and weight; greater size suggests heaviness and smallness suggests lightness, therefore, the over-compensation follows in the direction of the preliminary adjustment. The explanation may be clear enough to aid in understanding the illusory trend but is predictable only when normative investigations have verified the common pre-adjustment habit.

The question may arise whether or not the size of the average errors may be used as a constant correction factor to make accurate observations of such measures. Here they certainly cannot be used as a measure of precision, although they may have some crude correction value. If we note the large size of the semi-quartile range, we see the inaccuracy of such observations. The individuals are in the unskilled stages of the learning. The lack of meaning of the average of errors for those grossly lacking in skill is evident enough to preclude a precise correction constant. Knowledge of the average size of such errors cannot become a substitute for the necessity of checking accurately the observations made, or take the place of normal processes of learning. It, however, may somewhat short cut the learning or make slightly more accurate those observations which we do not intend to check. This inference probably will hold for illusions in general since they occur in comparable situations, namely, where skill is low and accurate checking is infrequent.

Question: 2.—Are there habitual differences in the direction or degree of the observations which vary with the form of the solid or container employed? The forms studied were: the cube; cylinders having a ratio of diameter to height of 1 : 1 and 1 : 2; a rectangular solid having the ratio of non-parallel sides of 1 : 2 : 4, approximately the general form of some drug store bottles; a jelly glass or paper cup pattern; and a globular pattern. In an objective series, the statistical comparisons were made relative to those cylinders, which approximated a 1 : 2 ratio and, therefore, the forms in the subjective series were compared with this form. The 1 : 2 cylinder was, also, on the whole, the most accurately estimated form. Table 2 shows the results relative to the 1 : 2 cylinder.

The differences between the 1 : 2 cylinder, the 1 : 2 : 4 rectangular solid, the jelly glass, and the globular forms are comparatively slight, but the 1 : 1 cylinder and the cubical form revealed an average departure from the 1 : 2 cylinder of over 60 percent of the volumes sampled at the median estimates, with 20 and 17 percent more of the group over-estimating the true volumes. These two forms, also, gave definitely wider standard deviations. Both forms tended to be more over-estimated, but the differences were not statistically reliable. The criterion of statistical reliability may not be wholly valid in indicating that no difference between the forms actually exists. In one very extreme case in this study, 100 percent of the group over-estimated the true volume and the difference between the median and the mathematically computed volume was 216 percent of that volume, and yet this difference was not as great as three times the standard deviation of the volume. If we are concerned only with the direction of the illusory trend in the latter example, we can be confident that the observations will be over-estimated but can say little about how much. So here, it is quite probable that the cubic and 1 : 1 cylinder forms are likely to be more over-estimated or, even less accurately estimated, but we are quite unable to predict the magnitude of the differences and hence cannot determine a correction factor with which to obtain accurate estimates and thus short cut to skilled responses.

TABLE 2

Forms	Diff. of vol. between Md.-Math.	Diff. in Q Size	Diff. Percent Overlap
Cube	+65	+127	+17
Cyl. 1 : 1	+60	+74	+20
Globe	+5.4	+20	+12
Rect. solid	+7	+4.9	∞
Jelly glass	−9.5	−8.5	+12

The cylinder 1 : 2 was taken as the basic form for comparison. Column 2 shows the differences in percentages of the volumes measured lying between the median estimation and the computed volumes, which are greater or less than for the 1 : 2 cylinder. Column 3 shows similar differences in the size of the semi-interquartile range. Column 4 shows average percents of the distributions reaching or surpassing the true volumes, which are greater than this percentage for the 1 : 2 cylinder.

Question: 3.—Are there fairly consistent habitual trends which are explainable as due to certain linear influences found in the volume situation? To illustrate, we might ask specifically, is there a tendency to over-estimate the taller cylinder? The experimental attack upon this problem involved the arrangement and presentation of seven different patterns of objects. In each set there were six non-uniformly graded solids. The fourth largest of these volumes was taken as the standard object and the other five, three smaller in volume and two larger were presented in a chance order, a single pair at a time, about 18 inches apart. Records were made in percents, fractions, or multiples of the standard, whichever was considered by the subject as most accurate for him. Four of the seven sets were cylinders, two of equal height, one relatively tall, and the other relatively short; one of equal diameters, and one which varied in both diameter and height. The other patterns were cubes, globes, and jelly glasses—one typical form for a paper cup. Table 3 gives data to support the following inferences which are based upon 350 observations approximately for each row in the table. It will be noted that only in one case is the difference between the median estimate and mathematically computed volume more than four times its semi-interquartile range. Only about 1/5th of the 35 samples could meet such a criterion of reliability. However, there is not a single median that is out of step with the inferences to be made. In more than 2/3rds of the cases, 75 percent or more of the distributions lie in that direction. In 77 percent of the cases the amount of the difference between median and computed volumes is 10 percent or more of the volumes estimated and in 25 percent of the cases 30 percent or more of their volumes. In 20 samples, there is a uniform tendency for wider diameters to be over-estimated. In the five taller cylinders of equal height, these differences are statistically reliable. When diameters are held constant, the common trend for the five cylinder comparisons is for the over-estimation of the taller heights. The effect of varying both cylinder diameters and heights gives a trend toward over-estimation, but the effect does not

appear to be cumulative. The jelly glass pattern, which is a sort of modified cylinder, gives results quite comparable to that of the last cylinder discussed.

The cubical and globular forms give a distinctly different picture. The direction in which the comparison is made must be noted. If the cubes and the globes of lesser volumes were compared with a larger standard, the lesser volume was over-estimated in the case of the cubical form and under-estimated in the globular; while if greater volumes were compared with a lesser standard, they were over-estimated in the case of the cubic forms and under-estimated in the case of the globular. The size of the under-estimations increased with the degree of divergence from the volume of one compared in the case of globes, and with a decrease in over-estimation with the degree of divergence for cubes. Only one of the actual differences in the case of the last two forms met the criterion of strict reliability.

TABLE 3

Pattern Constant	Vol. Ranges	Dia.-Ht. Ratio	Diff. between Md.-Math.	Md. Q	Ave. Overlap
Cylinder height	65–131	1.25	14	20	70%
Cylinder height	41–149	11.00	86	17	92%
Cylinder diameter	65–122	1.87	10	9	90%
Cylinder	22–182	1.00	12	17	75%
Jelly glass	40–131		7	9	75%
Cube	11–223	1.00	24	16	87%
Globular	20–1077	1.00	−12	18	27%

Column 2 shows the range of volume divergence from the standard or fourth largest solid in the series in percentage, the smallest volume listed first and the largest second, in terms of the percentage of the standard solid. Column 3 shows the diameter-height ratio of the standard solid, read as diameter 1 to given number. Column 4 gives the differences in percentages of the volumes compared between the median estimation and mathematically computed volume. Column 5 gives the semi-interquartile range in terms of percentage of the compared volumes. Column 6 gives the percentage of the distributions reaching or surpassing the computed volumes.

Question: 4.—If we can admit the plausibility of illusory trends pertaining to linear factors in the case of cylinders, can we expect that these will hold in direction for more extreme volume comparisons? In short, are there critical limits at or

beyond which they will not hold? A number of extreme comparisons was introduced to check the possibility of certain trends failing to hold beyond the limited range. The skill of the observers was further reduced by the extreme ranges to such an extent that only one difference between the median estimation and the mathematically computed volume gave a greater critical ratio than .5. The direction of trends found in the previous section, however, did hold for globes and cubes for an inadequate sampling for average divergences of 14 percent and 4 percent of the standard. For eight samples of cylinders, jelly glasses, and rectangular solids, the direction of the trends for wider and taller diameters failed to hold. The volumes ranged from 1 percent to 6.7 percent of the standard volumes, and the lesser volumes were uniformly over-estimated.

Table 4 gives some medians for the few samples 2–4 for each row, each sample including approximately 70 observations. It seems to be evident that trends should not be logically and mechanically assumed to hold over all possible ranges without experimental verification and that critical limits exist as a probability and hence should be investigated.

TABLE 4

Forms	Percent Vol.	Diff. Md.-Math.	Md. *Q*	Md. Overlap
Cylinders	5.0	−18.5	55	32
Jelly glass	6.2	−12	47	38
Rect. solid	3.9	−71	67	42
Cube	3.9	+ 1	27	85
Globe	14	−25	44	27

Column 2 gives the median percentage the smaller volume was of the larger. Column 3 shows the median percentage of the compared volume lying between the median estimate and true volume. The plus sign indicates that the larger volume was over-estimated, the minus that it was under-estimated. Column 4 shows the median semi-interquartile range in percentage of the compared volume. Column 5 shows the median percentages of the distribution for the larger volumes reachlng or surpassing the true volumes.

The determination of the magnitude of volume illusions not only has not been successfully determined in this study but also presents marked difficulty of determination. When

illusions are likely to be present, it is probable that the judges are relatively unskilled, hence variability and sources of error are likely to be large. The relative talents or skills of two groups may vary widely when skills are indifferently good, hence conflicting results may reflect merely non-homogeneity. The device of giving extensive training to subjects may be undesirable, if, as I suspect, we are dealing with unskilled portions of the learning curve in situations where illusions regularly prevail. Variations in the situation may cause considerable fluctuation, as may be noted by the modifications in the extreme comparisons in this study. In illusions of visual depth, variations which tend to decrease acuity or ability may observably enhance the magnitude of the illusion. Variations in the preliminary adjustments which the judges bring to the situation, or in the suggestive sets given by the experimenters may markedly influence estimations. An illustration on this point is revealing here. In conversation with a student I asked him to judge, not calculate, how many gallons it would take to make a cubic foot. He studied a moment and then said 8½. This was the first time that I had had an estimate greater than the computed measure of 7½, so I inquired how he reached that figure. He replied that from the way I put it, he felt that there must be some catch in it, so had judged it as well as he could, then had multiplied by 2.

In the past, the direction of illusory trends in simple situations has aided in understanding of the perceptual process and thrown light upon the basic reasons for the consistency of habitual trends in visual depth and similar situations. The knowledge of the direction of an illusory trend in a complex situation may be of great practical value as an aid in correcting estimates. In addition, such studies may aid increased understanding of the learning curve in its unskilled rather than in its final stages.

This investigation has revealed certain trends in the group studied. One should probably not assume that this sample is representative as to their position on the learning curve, or that their trends would hold outside moderate ranges. This

group did show a low degree of skill in estimating volumes. An accompanying questionnaire indicated that relative confidence was usually misplaced in regard to ability to judge which form of container could be more accurately estimated; the cubic form thought best was typically poorer than the 1 : 2 cylinder, for example. And the common practice of depending upon apparent size in judging price and volume requirements, showed the need of observing the printed quantity on packaged and bottled goods. The common illusory tendencies noted are that if a volume is considered relatively small, the tendency is to under-estimate its volume, if large, to over-estimate it; that forms having relatively wider diameters, are likely to be over-estimated; and that with cylindrical forms having equal diameters, the taller heights are likely to be over-estimated. The study, also, calls attention to the desirability of combining the simplified controlled laboratory researches and the naturalistic approach to practical complex situations. The two methods complement each other in verifying and validating conclusions.

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